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THE

JOURNAL OF SCIENCE.

JUNE, 1880.

I. INSANITY AND ITS DIFFICULTIES.*

DR. BEARD pronounces it "one of the paradoxes of psychology that we can best study the mysteries of the mind when that function is eclipsed by disease." Leaving to another opportunity, or perhaps to other hands, the discussion of the view here conveyed, that mind is merely a function, and not an entity, we see in the fact just mentioned, or rather in the exceptional facilities which it offers to the student, little that can be regarded as paradoxical, or even exceptional. For the successful investigation of any phenomenon whatever it is highly important that we should be able to view it under varying circumstances. But there is undoubtedly much that is paradoxical in connection with insanity. No subject, probably, of equal importance is so completely overlooked by the great mass of the educated public. Set on one side certain sections of the medical and the legal professions, a few philanthropists anxious that every practicable relief should be afforded to the victims of this scourge, and such private individuals as have relatives or friends in some asylum, and what cares the rest of the world about insanity? We no longer, indeed, like our powdered and periwigged forefathers, consider a visit to Bedlam as a holiday pastime. But little more than they do we in our turn recognise the terrible riddle which such institutions, Sphinx-like, propound for us to solve. We perhaps view madness merely as one of the horrors with which the sensational novelist of the day spices her† narratives, overlooking

* The Problems of Insanity: a Paper read before the New York Medico-Legal Society. By G. M. BEARD, A.M., M.D., &c. Reprint from the "Physician and Bulletin of the Medico-Legal Society."

† This department of literature is to such an extent in the hands of ladies that the feminine is here the more worthy gender.

the grim fact that among the poorer classes in England mental disease has during the last forty years been multiplied at the rate of 300 per cent, as against an increase of population of only 45 per cent. Nay, the late President of the British Psychological Association predicted that if the present growth of insanity continues at the same rate, we shall have, by the year 1912, a million and a quarter of lunatics in this our "highly favoured country." We believe that the estimate will rather fall short of than exceed the mark. We fear that even now the persons outside the pale of palpable insanity, but who are still guided by morbid emotions rather than by sound reason, and who are incapable of following a chain of causation of more than two links, may be counted by hundreds of thousands, and are consequently a serious and a formidable element in the community. What if the old saw "Quem Deus vult perdere" should prove to be literally true?

We do not, however, stand alone. Our kinsmen across the Atlantic are suffering, if less, only less than ourselves. France, Germany, Holland, Austria,—in fact Europe generally is moving in the same direction. So that not the British Empire alone, but modern civilisation as a whole, unlike that of classical antiquity, may perish, not from luxury, but from lunacy.

Surely, in face of such facts and of such prospects, not merely physicians and jurists, but society at large, might do well to seek out what are the causes of this growing plague, and what hope is there that it may yet be stayed.

Before asking after the cause of a phenomenon we shall do well to give it a definition, and here for all practical purposes we cannot do better than take the one which Dr. Beard offers:—"Insanity is a disease of the brain in which mental co-ordination is seriously impaired." This definition, it is true, does not stake out an absolute boundary line between sanity and insanity; but those who have studied Nature know full well that sharp antithetical classifications are in their essence not merely artificial, but delusive. Sanity shades gradually away into insanity, even as day into night and as each colour of the spectrum passes into its neighbour.

But the fact that we cannot run a hard and fast line between blue and green does not render us less able to distinguish them, and precisely similar is the case with mental health and mental disease.

Dr. Beard's definition of insanity has the further advantage that it makes no assumptions. Those who consider

mind as a mere function or result of the brain, and those who, like the present writer, recognise in it an independent entity, must alike admit the correlation of insanity with cerebral disease.

Madness, then, as an affection of the brain, is only one member of a class all of which seem to be decidedly on the increase—the diseases of the nervous system. What fosters this class? Why, in spite of our improvements in the medical art, of our sanitary reforms, our advances in domestic comfort, our more wholesome food, and our freedom from some of the excesses of our forefathers, does our vitality thus appear to be withering?

Not a few writers of the present day seek for the cause of increasing insanity in drunkenness, or rather in alcoholism, with which we may couple the use of opium, of ether, chloral hydrate, perhaps of tobacco, and even of tea and coffee.

Thus Dr. W. S. Hallaran* considers the use of whiskey as one of the constant and growing causes of insanity. Dr. W. A. F. Browne, of Dumfries, who had devoted much care and attention to the question of insanity, considers that close on one-fifth of the cases of derangement must be traced to intemperance. Earl Shaftesbury, after acting as a Commissioner of Lunacy for twenty years, concluded that 60 per cent of all the cases of insanity in the United Kingdom and in America "arise from no other cause than from habits of intemperance." This conclusion, it is but right to add, was uttered nearly thirty years ago, and consequently before the recent progress of insanity.

Dr. F. R. Lees† even declares that "insanity in every country corresponds in the main to the use of intoxicating drinks." In support of this proposition he gives a tabular view of the population per each deranged person in certain countries, in juxtaposition with the annual average consumption of alcohol per head. This table at first glance seems to support the theory in question, since, starting from Cairo, where there is one idiot or lunatic in every 30,714 persons, and where the annual average consumption of alcohol is null, we proceed to England, where the insane rise to one in every 713 persons, with an average yearly ingestion of alcohol to the extent of 3 gallons. But the question at once arises—Are there no other points, save the consumption of alcohol, in which the social conditions of

* Observations on Insanity. Cork: 1818.

† An Argument for the Legislative Prohibition of the Liquor Traffic. London: Tweedie.

England differ from those of Cairo? On looking further we find that the average consumption of alcohol is alike in the following countries:—Ireland, New York State, England, and Holland with Belgium, viz., 3 gallons per annum; yet the lunatics are in Ireland as 1 in 500; in New York State, 1 in 780; in England, 1 in 713; and in Holland with Belgium, 1 in 1046. In Southern France the consumption of alcohol (3½ gallons) is somewhat greater than in the places above mentioned, but the proportion of insanity is 1 in 1500 —in other words, more alcohol than is taken in Ireland leads to exactly one-third the mental derangement. Spain and Bretagne are alike in their small use of alcohol (1 gallon per head annually); but whilst Spain has only 1 lunatic in 7181 persons, Bretagne has one in 3500, or more than double; more even than Italy (1 in 3785), which consumes exactly twice as much alcohol.

Whilst fully recognising alcohol and its associates as ranking among the causes of insanity, we fail to see that it plays the predominant part which is assigned it by the leaders of the Temperance Movement, or that it is in any especial manner responsible for the fearful increase of mental disease during the last forty years.

Dr. Beard, both from the observations of others and from his own investigations, concludes that excess in narcotics and stimulants, sensual indulgence, &c., are powerless to produce insanity in any considerable extent. He has visited the so-called Sea Islands between Charleston and Savannah, —famous for their superior cotton,—and has there studied the Negro population, who have scarcely been brought into contact with civilisation, and who intellectually are little in advance of their African ancestors. He declares that "All the exciting causes which philosophers have assigned as explanations of insanity, and of its increase in civilised countries, are operating there with constant and tremendous power. These primitive people can go, when required, for weeks and months sleeping but one or two hours out of the twenty-four; they can go for all day, or for two days, eating nothing or but little; hog and hominy and fish, all the year round, they can eat without getting dyspepsia; indulgence of passions, tenfold greater at least than is the habit of the whites, never injures them either permanently or temporarily; alcohol, when they can get it, they drink with freedom, and become intoxicated like the whites, but rarely indeed manifest the symptoms of *delirium tremens*, and never of chronic alcoholism." And what is the result of such lives? Dr. Beard shall speak again:—"There is almost no insanity

among these Negroes; there is no functional nervous disease or symptoms among them of any name or phase; to suggest spinal irritation, or hysteria, or hay-fever, or nervous dyspepsia among these people is but to joke. . . . Of nervous diseases, from insanity down through all the grades, they know little more or no more than their distant relatives on the banks of the Congo." These people, it is added, are "types of all Central Africa, of South America, of Australia, of our not so very distant ancestors in Europe." So much for the alcohol and general excess theory!

Another school of thinkers trace the modern increase of mind-diseases to the decline of religious faith and the spread of indifferentism, or even of what is conventionally known as scepticism. It has even been said that insanity is not so much disease as sin, and that no one who retains a firm trust in God need fear becoming its victim. It has further been declared that mental alienation is less abundant in Catholic than in Protestant countries. The table of Dr. F. R. Lees, referred to above, lends quite as much support to these views as to the alcohol theory. Take the two most decidedly Catholic countries in Europe, Spain and Italy. The former has only 1 lunatic in 7181 persons, and the latter—where scepticism has made some progress among the upper classes—1 in 3785. The northern provinces of France, where anti-clericalism and irreligion have their stronghold, number 1 insane person per 1000; the more Catholic south, 1 in 1500; and devout Bretagne, only 1 in 3500. But Ireland, also a Catholic country, with its 1 insane person in 500, is a fact in opposition which cannot be overlooked. Nor can we ascribe to religious faith that immunity from mental disease which savage and semi-savage races—such, for instance, as the Sea Island Negroes above mentioned—evidently enjoy.

We come now to Dr. Beard's own theory, startling and perhaps exaggerated, but containing at any rate such a basis of truth as to constitute the most tremendous impeachment of modern civilisation ever conceived. If we survey the whole world we shall find that just in proportion as a nation, instead of working to live, lives to work; just in proportion as life becomes a struggle, a race, a scurry; as the margin which separates the individual man from actual want is narrowed, and as anxiety becomes the normal frame of mind of the community, in that same proportion does insanity, and indeed the entire class of nervous diseases, increase. Dr. Beard asserts that "no climate, no institutions, no environment, can make insanity common except when united

with and reinforced by brain-work and in-door life." He ought, we think, to have here introduced a qualification—"brain work carried on under the influence of anxiety." No one has shown more fully and clearly than has Dr. Beard himself that intellectual work is in itself conducive to health and long life. Take the case of a man who can enter upon a course of scientific research, not depending upon its results for his livelihood, not compelled to complete his investigations by any fixed date; neither his bodily nor his mental health will be endangered; but compel a man to think, and discover, and invent, with the penalty of want hanging over him in case of failure, or merely delay, and we need not be surprised if his brain gives way. Of all the causes which debilitate the nervous system anxiety bears the palm.

Dr. Beard considers that there are "five features of the nineteenth century civilisation that are peculiar to it, unprecedented in history—the printing-press, the telegraph, steam-power, the sciences, and the mental activity of woman." He might have added, at least for England, "competitive examination." All these agencies, by increasing anxiety and worry, have become causes of insanity. "The telegraph alone has multiplied manifold the friction of life." Standing where we now do, in the last quarter of this nineteenth century, it is strangely suggestive to contrast the realities around us with the anticipations entertained fifty years ago as to the social results of mechanical inventions. They were to lighten toil, to give men leisure to think, in other words leisure to live; in short to make life easier. In the "Song of the Steam-Engine," written by a poet of the "Good Time Coming" school, this wonderful product of human ingenuity is made to say—

> "And soon I intend you may go and play
> While I manage the world myself!"

We need not waste ink and paper in showing how completely these expectations have been disappointed. But concerning the effects of the modern study of Science our author seems at times scarcely self-consistent. We find him here declaring that "the modern brain must carry and endure tenfold more than the ancient, and without a correlated increase of carrying and bearing force. Whence comes insanity, with its train of neuroses." Again, he recommends, among our prime needs for the arrest of the multiplication of the insane, "a partial reversion to the calmness and ignorance of our ancestors." To their calmness, say we, not merely partially, but totally; but to their

ignorance? Nay, without referring to what Dr. Beard has written in his "Longevity of Brain-Workers,"* we find him in the memoir before us ranking among the means for preventing nervous disease "the development of the intellect at the expense of the emotions, which is the tendency of the age in all highly civilised countries." Again, "We are made nervous and kept nervous through our emotions, and kept well by the activity of the intellect." If so, why is "the intellectual activity of the modern woman" pronounced to be "a potent and slightly considered cause of insanity." If, as Dr. Beard fully admits, barbarians and savages are much more emotional and less intellectual than ourselves, and at the same time free from insanity, why should we expect to be freed from mental disease by a further repression of emotion and cultivation of the intellect? How does this prescription agree with the recommendation given a few pages back, to return to the ignorance of our fathers?

Why, again, if insanity is so rare among the semi-savages and barbarians of inferior races, should it be—as we find it here stated—so prominently abundant among the poorest and most degraded classes of our great cities? That their condition is more miserable than that of the savage we admit as decidedly as does Dr. Beard. Let us listen to his description of their lot:—"Civilisation grinds hardest on the poor, for it deprives them of most of the pleasures and delights and healthful influences of barbarism, without the compensations that the higher classes of civilisation enjoy; it shuts them up in houses, gives them bad air and bad food and all phases of bad environment, crushes them, and keeps them crushed by rivalry and competition, corrodes them with envy, and gives them what many barbarians cannot get, *stimulants and narcotics in indefinite quantities at a moderate price and of the most possible easy access.*" We have not space to quote at greater length from this passage, which deserves to be pondered over by every philanthropist, clergyman, physician, and, most of all, by every legislator. But we must ask, where in the condition of these classes do we find the "brain-work," without which the author has declared that no other agencies can make insanity common. We call attention, too, to the words we have italicised, which seem to convey a half admission of the alcohol theory of insanity.

Dr. Beard's proposals for a revision of our school and college systems are well worthy of attention. He is as

* Quarterly Journal of Science, vol. v. (1875), p. 430.

hostile to cram as is the "Journal of Science," and in face of the overwhelming amount of the knowable he evidently leans, like ourselves, to Specialism, and would not examine the curator of a zoological museum in the higher mathematics and in dead languages.

II. THE HISTORY OF ANTOZONE AND PEROXIDE OF HYDROGEN.

By Albert R. Leeds, Ph.D.

I. *Antozone.*

By far the most important fact in the long and perplexing history of antozone is the recent discovery that there is no antozone. After giving rise to a very voluminous literature, filled with confused and contradictory statements, the mysterious body named by Schönbein "Antozone" has disappeared from the pages of chemistry, and been added to that daily increasing host of defunct chemical elements which, after a brief and troubled existence, have fallen into final oblivion. As it was, it never had a sturdy existence. It appeared to be a sort of chemical Will-o'-the-Wisp, a matter of exhalations, connecting its existence with the formation and disappearance of clouds and similar phenomena, and ever resisting the attempts of the experimenter to obtain it in some tangible form. The ghost of antozone, raised by Schönbein, and, together with its twin-brother, atmizone, expanded into great proportions by the labours of Meissner, was struck down by Von Babo (in his "Contributions towards a Knowledge of Ozone," 1863), and finally laid by the experiments of Nasse and Engler, on the phenomena attendant upon the action of oil of vitriol upon peroxide of barium (1870).

And when we consider for a moment the overwhelming host of acquisitions which are being yearly made to our stores of veritable chemical knowledge, the mind experiences a sensation of actual relief in seeing so many questionable statements expunged from the history of chemistry, and in getting hold, so to speak, of an unexpected *tabula rasa* on

which to write discoveries of permanent value. Such being the case, it would certainly be an unremunerative toil to weigh and ponder the great bulk of conflicting data concerning antozone. The most we would feel willing to undertake would be to inquire into the grounds upon which such mistaken views were originally built; further, into the experiments which appeared to confirm these views, and eventually to win for them the credence of philosophers in general; and, finally, to examine narrowly into the validity of the experimental evidence which is regarded as demonstrating conclusively the non-existence of antozone.

The ground was prepared for the growth of a belief in the actual separate existence of antozone by the promulgation by Schönbein of his theory of Ozonides and Antozonides. Under the former class he included the peroxides which, in their action upon other bodies, manifested a strong likeness to ozone, the typical body of this class being the peroxide of lead.

Without enumerating all the features in their deportment towards other bodies, an enumeration which would serve only to confuse us, it will be sufficient for our present purpose to note that the properties of ozonides that Schönbein regarded as most characteristic were their power of liberating chlorine on contact with hydrochloric acid; of being reduced by peroxide of hydrogen to lower oxides (water and ordinary oxygen at the same time being generated); and of causing the tincture of the resin guaiacum to turn blue.

Antozonides, on the other hand, were those peroxides which, under the circumstances detailed above, behaved in quite contrary fashion,—under no circumstances liberating chlorine from a chloride, not decomposing peroxide of hydrogen, and not turning guaiacum tincture blue. The typical body of this class was peroxide of barium.

And inasmuch as Schönbein thought he had demonstrated that ozone is electro-negative oxygen, and that the ozonides were combinations of a lower oxide with ozone, he accordingly regarded the antozonides as combinations of a lower oxide with electro-positive oxygen. This electro-positive oxygen he appears to have named antozone to distinguish it from ozone, and to indicate the function it performed in antozonides, without claiming, at least at the outset, that it had been or could be isolated in a free condition. The fact that an ozonide and an antozonide could naturally decompose one another, and both at the same time undergo reduction to the state of lower oxides with liberation of ordinary oxygen, was regarded as lending great probability to the

view that the oxygen in the two compounds existed in two opposed electro-chemical conditions.

This hypothesis of Schönbein was evolved at that epoch when the electro-chemical theories of Berzelius reigned paramount, and has the same general objection which is urged against the dualistic theory in general, that, instead of regarding a chemical compound as a new individual in which for the time being the specific identity of its components are lost, it assumes that these components, though unrecognisable for the time, nevertheless still exist. In other words, that in an ozonide there is ozone in combination with a lower oxide, and antozone in an antozonide. The validity of this reasoning is denied on the ground that a compound body may yield up its constituents in one form or in another form, according to the reagents, or according to the circumstances, &c., by which its decomposition is brought about. So with the bodies under consideration. It was pointed out by Brodie (1863) that the chemical differences in the deportment of the ozonides and antozonides were to be attributed to the nature of the substances with which in each class of bodies the oxygen was united, and to the nature of the substances taking part in the reactions, rather than to the existence in them of two different modifications of oxygen. For example, taking the evolution of chlorine when a chloride is brought into contact with an ozonide as the most characteristic of its properties, as was done by Schönbein, we certainly should not anticipate that peroxide of barium, which is the typical antozonide, in contact with chloride should evolve chlorine. But it was shown by Brodie (1861) that it did so or not according to circumstances—with concentrated hydrochloric acid, yielding chlorine; with dilute, peroxide of hydrogen.

In the same direction tended the still earlier observations of Lenssen, that peroxide of hydrogen (an antozonide) could add oxygen to, or subtract oxygen from, an oxidisable body, according as the circumstances of the reaction, or as naturalists at the present time are fond of saying "the environment," are favourable to the formation of a higher or a lower stage of oxidation. Thus in alkaline solution it oxidises oxide of chromium to chromic acid, while it reduces chromic acid to oxide in the acid solution. The above facts are irreconcilable with the hypothesis that an ozonide contained ozone as such,—and an antozonide, antozone; consequently the hypothesis and with it the terms employed have been abandoned.

But the existence or non-existence of ozone is not only

independent of the truth or falsity of any such hypothesis, but its properties have been studied with a minuteness and exactitude that render it in fact a much better known body than either sulphur or phosphorus. It is questionable whether or no sulphur and phosphorus are elementary bodies; but no one doubts that the substance-matter of ozone and ordinary oxygen is identical, and the relations existing between these allotropic conditions of one and the same elemental substance are clearly and sharply defined. How does the case stand with *antozone?*

It is manifest that the theoretical speculations of Schönbein upon the existence of electro-negative and of electro-positive oxygen, in a state of combination with lower oxides in ozonides and antozonides respectively, would strongly incline him to the possibility of obtaining, in a free state, antozone, corresponding to the previously obtained modification of oxygen, ozone. Accordingly we find later that Schönbein thought that the gas set free by the action of oil of vitriol on barium peroxide contained antozone. He likewise formulated a number of characteristics by which the presence of antozone could be recognised. Without pausing to enumerate all of these, it will be of service to us, in obtaining a clear conception of Schönbein's conception of antozone, to specify the three most salient. They are—1. That antozone, such as is made from barium peroxide, combines with water to form peroxide of hydrogen; ozone, on the contrary, cannot oxidise water to the form of peroxide. 2. It does not turn manganous salts brown, while ozone does, a higher oxide of manganese in the latter case being formed. 3. It bleaches paper saturated with manganous and lead salts, after they have previously been turned brown by ozone. Unfortunately these marks of distinction were open to sources of mistake in their verification. But had the antozone been odourless, or incapable of turning iodo-potassium starch-paper blue, Schönbein would have stated grounds of difference which would have rendered it possible readily to distinguish between it and ozone. On the contrary, in these two most striking points, according to Schönbein, antozone and ozone were nearly alike.

Perplexing as the subject was rendered by the numerous, and not unfrequently the contradictory, statements of Schönbein, it was enveloped in a far more disheartening nebulosity, and—it is hardly exaggeration to say—buried beneath a dense fog raised around it by the indefatigable and life-long labours of Meissner. Witness the following samples of Meissner's modes of conceiving and stating the

nature of the problems under study, and ask yourselves whether, as he stated them, the problems were not too vague to admit of precise thinking or of crucial experimentation. Antozone, says Meissner, is identical with the gas which is set free by the action of sulphuric acid upon peroxide of barium, except in the two respects that, unlike this gas, it does not decompose iodide of potassium, and it does not smell. (In other words, it is identical with a gas from which it differs in two essential characters.) But (note how the accompanying qualification tends to clarify our ideas) this gas likewise loses its smell, clouds at the same time being formed, on coming into contact with moist air. (!)

According to Meissner ozone could not oxidise nitrogen, and probably antozone alone could not do so either, but both together could bring it about, in case moisture were present and other oxidisable bodies were absent. As the peculiarly distinguishing characteristic of antozone, Meissner rated its power of forming clouds in contact with water. When the water was abstracted from these clouds by contact with desiccating bodies, the dried antozone could form antozone again by transmission through water.

Finally, in opposition to Schönbein, Meissner held that antozone was not absorbed or acted upon by potassium iodide, so that if a mixture of ozone and antozone is passed through a solution of iodide of potassium the ozone is absorbed, while the antozone escapes and passes on free.

I have endeavoured to present above the views entertained by Schönbein, Meissner, and others concerning antozone, as lucidly as the contradictory and oftentimes vague statements made concerning it would allow, and have brought its history down to the time of the publication by Von Babo of the memoir before alluded to (1863), in which the weakness of the experimental evidence brought forth in support of a belief in its existence was for the first time clearly set forth. For Meissner, it will be recollected, saw, in its power of generating a cloud in contact with water, the distinguishing property of antozone. Von Babo discovered that the formation of a cloud is always to be noted when, in any manner whatsoever, ozone is decomposed, water being present. Meissner believed that the clouds could not be due to peroxide of hydrogen, because, according to him, the latter is not volatile. If, then, peroxide of hydrogen was not concerned in these phenomena, there was left—as the only other alternative under the circumstances—the hypothesis of a peculiar modification of oxygen capable of giving rise to them; and to this modification, which again was

necessarily different from ozone, Meissner gave the name of Atmizone. Later he identified it with, and called it by, the same name as Schönbein's, Antozone. Von Babo, on the contrary, found that the clouds were only peroxide of hydrogen diffused through vapour of water, and capable of being transported along with it, and even passing with it through aqueous solutions for long distances, without being deposited or absorbed.

Unfortunately these results of Von Babo were encumbered with certain vague and doubtful speculations concerning the mode of genesis of the peroxide of hydrogen, through the interaction of ozone and water in the presence of an oxidisable substance. That they were in reality conclusive against the existence of the so-called antozone was not generally recognised until the labours of Nasse and Engler (1870), upon the gas set free by the action of sulphuric acid upon peroxide of barium, had confirmed their truth and illuminated their proper bearings and significance. Nasse and Engler, by simple and trenchant experiments, demonstrated that the gas evolved in this case was a mixture, containing not only ozone, but also water and peroxide of hydrogen. When the escaping gas was passed through a series of tubes surrounded with a freezing mixture, the latter underwent condensation, and the permanent gas which passed on was ozone. The condensed product, when subjected to appropriate tests, proved to be merely a solution of peroxide of hydrogen. Carry the simple explanation thus afforded with you, and see with what a flood of light it illuminates all the hitherto hopelessly obscure passages in the history of antozone, and enables one to give readily a natural explanation to phenomena which at the time of their original discovery perplexed mightily their discoverers, and led them to form many ingenious, but in the end harmful, hypotheses.

For instance, examine, with the aid thus given, Schönbein's first distinguishing characteristic of antozone, *i.e.*, as made from barium peroxide it combines with water to form peroxide of hydrogen. Since the gas given off in this reaction consists not only of ozone, but of peroxide of hydrogen, the peroxide of hydrogen which Schönbein thought was formed on its coming into contact with water really pre-existed. Consider his second test—that antozone does not turn manganous salts brown, while ozone does. This difference is likewise true of peroxide of hydrogen as compared with ozone. The same remark applies to his third test—that it bleaches papers saturated with manganous salts, after they

have been turned brown by ozone. The same effect precisely is produced by peroxide of hydrogen. Is there any adequate explanation of these agreements short of conceding that Schönbein's antozone is disguised peroxide of hydrogen?

But what shall we say of those numerous cases in which Meissner thought that a mixture of ozone and antozone was present, and that on removing the former, by passing the mixture through a solution of iodide of potassium, the latter went on alone, attended with its characteristic white cloud? The explanation is that afforded by the experiments of Von Babo: when ozone decomposes potassium iodide solution there is formed, in addition to free iodine, iodate of potassium, and potassium peroxide, *peroxide of hydrogen*. If anyone doubts the adequacy of this explanation, let him try the following experiment:—Strongly ozonise some *dry* oxygen by an electrical ozoniser; pass the ozonised gas through a sulphuric acid wash-bottle, and then allow it to descend upon a potassium iodide solution. The ozone will undergo complete absorption, the solution becoming deeply coloured by the liberated iodine. Resting upon the surface will be seen a dense white cloud. This white cloud may now be aspirated through many wash-bottles containing water, and even a solution of chromic acid, and may stand for hours over water before it completely disappears. But on examining the waters used in washing it, they will be found to contain peroxide of hydrogen. Apply the same mode of solving the other statements made by Meissner, remembering always that the peroxide of hydrogen which is formed when ozone is decomposed by an aqueous solution is attended by a white cloud, through which the peroxide of hydrogen has diffused itself,—a white cloud of such permanence that it may be transmitted through many solutions before undergoing absorption,—and their explanation will be found both natural and easy.

In conclusion, why not make an end of the matter by stating that antozone is peroxide of hydrogen? The objection to so doing is, that along with the term antozone there were attached many notions which are not true of peroxide of hydrogen, such as its being electro-positive oxygen, that it had the power of forming peroxide of hydrogen on coming into contact with water, &c. Finally, the very name antozone implies a substance in its nature the opposite of ozone, and supposes the existence of a theory to account for the difference. For these reasons I deem it more just to sum up the question by re-affirming the

affirmation made at the beginning, that *there is not, and never was, antozone.*

II. *Peroxide of Hydrogen.*

Though peroxide of hydrogen was discovered by Thénard more than half a century ago (1818), and has ever been a substance possessed of unusual interest in the eyes of chemists, yet the difficulties of its manufacture were so great that only recently has it ceased to be a chemical curiosity and come into use in the arts. Only a year ago a very dilute solution of the peroxide, imported from Europe, was sold in New York at the price of 16 dollars per gallon. But to-day a solution containing 8 to 10 per cent is retailed at about 1 dollar per lb. At this high price it is sold under fanciful names, and employed to bleach the hair, being used —as Dr. Warren would have styled it—as an Anticyanochaitanthropopoion or Tetaragmenon Abracadabra, to change the dingy tresses of the titmice among the ladies to a ravishing tow. But there is much reason for believing that a most important future is before it; and whether in the chemist's laboratory or in the arts, as a most powerful oxidising and reducing agent,—for it can act as both,—for bleaching purposes, &c., it is destined to play a great part. With its cheapening, many new uses will be found for it, and it is probable that before very long it will take its place, as Mr. G. E. Davis has strongly urged ("Chemical News," xxxix., p. 220), as an indispensable article upon the working table of every chemist.

But it is not these considerations which mainly interest us in connection with its scientific history. It is rather the accessions to our knowledge, which, more especially of late, have elucidated many obscure points connected with its sources and properties. That the method of preparation from peroxide of barium and hydrochloric acid (Thénard, 1818), or from the same oxide and carbonic acid (Duprey, 1862; Balard, 1862), is not used to obtain it on a commercial scale, is familar to many, the method of Pelouze, in which hydrofluoric or fluosilicic acid is employed to effect the decomposition, being that employed in the arts.

That peroxide of hydrogen was formed in the electrolysis of water strongly acidulated with sulphuric acid was stated by Meidinger (1853), and was apparently so well confirmed by the experiments of Bunsen (1854), C. Hoffmann (1867), and others, that until the researches of Berthelot (1875) were published the production of peroxide of hydrogen in electrolysis was looked upon as a fully established fact.

But the great French chemist showed that the body dissolved in the acid electrolyte did not exhibit the reactions characteristic of peroxide of hydrogen: it did not decompose potassium permanganate (Brodie's test), nor oxidise chromic to perchromic acid (Barreswil's test), nor convert calcium hydrate into an insoluble peroxide in alkaline solution (Berthelot's test?). He demonstrated that it contained in solution the same oxide of sulphur which he had previously formed as a beautifully crystalline body by the long-continued exposure of dry ozone and dry sulphurous acid to the action of the silent electric discharge,—Berthelot's persulphuric anhydride, S_2O_7. Finally, during the course of the year just passed, Schöne has demonstrated, in his elaborate research upon the behaviour of peroxide of hydrogen towards the galvanic current (1879), that *in the electrolysis of water no hydrogen peroxide is formed.*

Will the same be found to be true of Schönbein's statement, that in the oxidation of phosphorus exposed to moist air, along with ozone, a by no means inconsiderable quantity of peroxide of hydrogen is formed? This point was investigated by the author in the course of a research into the by-products obtained in the ozonation of air by phosphorus, with the result of confirming Schönbein's observation. The amount of hydrogen peroxide was determined by analysis of the water employed in washing the ozonised gas, the iodine liberated by the washed gas being attributed entirely to the decomposition effected in a neutral solution of potassium iodide by the ozone. The proportion of hydrogen peroxide to the ozone, as determined by this method, was only one to four hundred. But later the author has investigated the subject, estimating not only the hydrogen peroxide held back in solution, but the entire amount present in the ozonised gas, and has found that its proportion to that of the ozone may exceed one to three. The two substances, as Schöne has pointed out, may be present in the same vessel in quite a concentrated form, for a long interval, without effecting a complete mutual decomposition, and when highly dilute may coexist for hours.

One question of very great interest still remains:—*Is peroxide of hydrogen present in the atmosphere?* As yet, except as an inference from other meteoric phenomena, there is no evidence that it is. Meissner (1863), Schönbein (1868), Struve and Schmid (1869), and Goppelsröder (1871), believed that they had succeeded in demonstrating the presence of peroxide of hydrogen in rain. Houzeau, whose authority in matters of chemical climatology no one would feel disposed

to question, seeing that he gave a lifetime of arduous study to their elucidation, made very numerous analyses of the atmospheric precipitates at different seasons of the year occurring in the vicinity of Rouen (1868); but he did not succeed in finding peroxide of hydrogen either in snow- or rain-water, nor in natural or artificial dew.

But in the year 1874 Schöne made an elaborate investigation of the subject, and obtained results which established that—in that locality at least, and at the time his experiments were performed—hydrogen peroxide was present in certain atmospheric precipitates. Of 130 samples of rain-water, collected during the latter half of the year 1874, at Petrowskoje, near Moscow, he found only four in which hydrogen peroxide could not be detected. Of snow, of which twenty-nine samples were examined, there were twelve in which the presence of hydrogen peroxide could not be proven. As to the amount, Schöne found that it varied in rain-water between *one part in one million to one part in twenty-five millions.*

The problem how to detect with scientific certitude the presence of ozone, or peroxide of hydrogen, or both, in the excessively dilute condition in which—if ordinarily they exist at all—they must be present in the earth's atmosphere, is still unsolved. And while its importance, as a leading factor in chemical and medical climatology, is on all sides generally admitted, there appears to be scanty prospect of its speedy or satisfactory settlement.

III. THE ORIGIN OF FALLING MOTION.

By Charles Morris.

WHY do bodies fall? The attraction of gravitation may be the active cause of their passing from a state of rest into a state of motion. But attraction of gravitation does not create this motion. Nor can we well imagine gravitative energy to be a mode of motion convertible into other modes. However great the effect produced the force of gravitation remains unchanged. It is not transformed into motion of masses.

Whence, then, arises this motion? It is a form of energy, and must be derived from some diverse form of energy which it replaces. If, for instance, a body begins to fall to the earth from a position of rest, we can safely assert that the motion it displays pre-existed, either in the earth, in the body, or in surrounding space. It was certainly not created for the occasion.

The theory of gravitation declares that the earth moves towards the falling body with a momentum equal to its own. If the body be supported above the earth, the support performs a double duty. It at once hinders the body from falling to the earth, and the earth from falling to the body. They compose parts of one rigid system. But if the support be removed the earth and the body at once become separate individuals, and they fall together, with equal momentums, until they again enter into rigid relations with each other.

The falling motion manifested by the descending body cannot, then, have been in some mysterious manner transferred to it from the earth; for the earth's own motion is equally to be accounted for, and in that case we would have to look to the body for its source. No active motion could appear in such an equal mutual transfer of motive vigour. We must therefore look elsewhere for the source of the motive energy displayed.

Nor can it well have been derived from contiguous space. It is too instantaneous in its appearance, and too regular in its increase, to arise from any such transfer of moving energy.

It must therefore have had its origin in the moving bodies themselves. Not, however, as an ideal "potential energy" converted into a real "actual energy;" but as a real motion, existing previously in some other form, and converted as needed into the form of mass motion.

Such motive energies exist as constituent forces of all matter. They present various modifications, and are named electrical, magnetic, chemical, cohesive, and temperate energies. These are partly modes of motion, partly modes of attraction: they are specialised manifestations of the general attractions and motions native to matter. The generalised form of attraction we possess in gravitative energy. The specialised forms are the organising attractions of substances, such as cohesion, chemism, and possibly magnetism. It is the same with motions. The generalised form is the free movement of gas particles; the specialised forms are electricity, and heat as it exists in liquids and

solids. But these two modes of motion are differently related to masses. Electricity is an organising energy. It only manifests itself through change in the organisation, or in the relations of bodies. Its only ready transformation is into heat.

Heat is a disrupting energy. It is the individual energy of the separate particles, and has nothing to do with the organisation of molecules into masses; yet it is a generalised condition of motion only as it exists in gases. In liquids and solids it appears to be partly specialised; most probably becoming some form of rotation in liquids and of vibration in solids.

Heat force is neither concerned in the organisation of the mass, nor is it closely related to the particle containing it. It is capable of ready transfer from particle to particle, and of ready change in direction.

We may look upon every separate molecule or distinct particle of a solid body as dwelling within a nest of attractions. The fixed organisation of the body most probably causes these attractions to become definite in direction, so that it is not improbable that the motion of each particle is confined to a fixed centre upon which these attractions converge, through which centre it must vibrate, or around which it must rotate.

But the forces acting upon the particle are not alone the attractive energies and the repulsive impacts of contiguous particles. The attractive or gravitative energy of the earth is also a powerful factor in the result. This energy must influence the direction in which the particle moves. It is therefore one of the various active forces to which this direction of motion must conform.

And gravitative energy is constant in vigour and direction. It does not vary as the forces of the surrounding particles may do. Thus every vibration or other movement of the particle has a vertical component, in response to gravitation, which must exercise a constant and unvarying influence upon the result.

Every particle, in fact, is incessantly falling. What we call a position of rest is really a position of constantly-arrested fall. If the surrounding attractions tend to force the particle towards a fixed point in space, the attraction of gravity tends to force it below this point. Thus it never moves to the exact point required by its contiguous attractions, but to a point nearer the earth, which forms a centre of all its attractions, that of gravitation included.

The distance between these two points is the distance to

which the particle falls during every vibration. It is arrested at this point by the surrounding attractions, the real and ultimate arresting force being the repelling impact of the particles of the supporting substance.

Every downward movement of the particle is thus aided by gravitative attraction. Every upward movement is retarded. These invigorated downstrokes become themselves an element in the problem; they add, by their impacting force, to the descending energy of the particles below them. Therefore the lower plane of particles manifests the combined gravitative energy of all the particles of the mass. This is what we call weight, this energy of impact, produced by gravity, of the particles of every substance upon its support. It constitutes an incessant rain of down-beating particles: they strike downwards with a vigour depending, primarily, upon their own response to gravity; secondarily, upon the gravitative pressure of the particles above them. The support must be strong enough to bear its load, or it will inevitably give way under this fierce and incessant rain. If the support be removed, what follows? The forces surrounding the particle remain the same. It descends in response to the gravitative component of these forces. This descent is not resisted by the surrounding energies, since all the particles descend at the same time from the same cause. The only real resistance to fall is the upward compact of particles occupying the space through which the fall must take place. If this resistance be removed or sufficiently decreased every particle of the mass must simultaneously descend in response to gravity, and the whole mass change its position.

Thus the heat movements of the particles are made to conform in direction by the attraction of the earth, this conformity constituting a movement of the mass as a whole. And this is a regularly increasing movement. As the mass moves its motion constitutes an energy. The motion caused in each instant by gravitation remains the same. But it has a separate effect in every separate instant, and these effects are persistent and constantly accumulate, producing a regularly increasing motion of descent.

Falling motion, then, appears to be a partial specialisation of the heat movements of the particles. These movements are made to conform in direction to a certain degree, under the influence of a fixed and persistent attraction. This descent is continuous, whether it be resisted or not. If resisted it cannot accumulate. Each momentary fall makes itself felt as weight by the resisting body; but these

momentary falls are each obliterated by a reverse impact, and cannot be added together, constituting an increasing energy of fall. Only when the resistance is removed, and the particles are no longer driven back by impact, does the falling energy manifest itself in a downward movement of the mass.

Thus, when a body falls, part of its heat motion has been transformed, and has become motion of the mass as a whole. The generalised motion of heat has become partly specialised into motion of the mass. This is readily transformable again into heat; but it can only be so transformed by resistance. It is persistent as mass motion until some resisting energy overcomes it, when it again becomes heat.

And from this fact two conclusions necessarily arise. The first is, that a body whose mass motion is resisted must display an increase of temperature. The conformity in the motion of the particles is broken; they again move individually instead of collectively. Temperature effects appear in consequence.

The other conclusion is, that a body yielding to gravitation, in increasing its mass motion, must decrease in temperature. Its temperature is being converted into another form of force, and cannot continue to display its usual effects. The body grows colder in every direction except that of its mass motion, the movements of the particles being specialised in this direction, and their impacting force partly decreased in all other directions.

The heat thus lost, as heat, is probably regained from the radiations of the matter through which the body moves, so that its sum of forces is increased in consequence of a special transformation of a portion of them.

Where the motion of the body is decreased or increased by gravitation, without radiation of heat from other sources, certain interesting and perhaps important effects must ensue. If a mass be driven upward against gravitation its particles must continue to fall. The downstroke of their vertical component of motion, as caused by gravitation, is constantly more vigorous than the upstroke. The fall of the body is simply masked by its upward motion, and accumulates in the same manner as if the mass was descending. Thus the upward motion is more and more rapidly obliterated, and soon ceases to exist, the mass becoming momentarily at rest. What has become of the mass motion? Evidently there has been a simple change in the character of the motion of the particles. Instead of moving upwards more rapidly than they descended, they now move upward and

downwards with the same vigour. The special mass motion has fallen back into the body, and has become vibratory movement of its particles. It has, in fact, become temperature, and the sum of temperature energy has increased through this loss of mass motion.

If now we apply this idea to the movement of the planetary bodies, some interesting deductions may be made. In the case of a comet moving from the sun we have an exact counterpart of that of a body thrown upward against gravity. The particles of the comet continue to fall towards the sun. These slight falls are masked in the mass motion of the comet, but they slowly consume this motion. They constantly accumulate, precisely as if the reverse motion did not exist. The comet is thus at once moving outward from the sun and falling inward to the sun, and its real motion is the difference between these opposite energies. Its mass motion is, in short, falling back into its substance, and becoming vibratory motion. Eventually the fall increases in vigour until it equals the outward motion of the mass. At this point the comet ceases to remove from the sun. The outward and inward movement of its particles have become equal; the vertical component of their motion through space has become converted into a vibration about a fixed point in space. It has, in fact, become heat motion.

Thus the strange fact displays itself of a rapidly-increasing temperature in the comet, as a necessary consequence of its movement outward from the sun. In its return to the sun the opposite effect occurs. Its vibratory motion is gradually transformed into mass motion. Every new increment of mass motion thus gained is at the expense of the heat vibration, and the temperature necessarily decreases in consequence.

This effect is, of course, masked in its increased reception of radiant heat in approaching, and its rapid radiation into space while leaving the sun. It is in this like a falling body whose lost temperature is regained from the radiations of surrounding matter.

A precisely similar effect must occur in the case of every planet which has an elliptical orbit. The earth, for instance, after passing its perihelion point, begins to move outward from the sun, against gravitation; but the fall of its particles towards the sun at once tends to consume this outward movement. The earth possesses really three movements, from whose composition its orbital movement results. One of these is a movement at a tangent to the radius of its orbit. This is resisted by a falling motion towards the sun,

in response to gravitation. These two energies are exactly balanced; neither can accumulate at the expense of the other, and they result in a circular orbit. But there is a third motion, a vertical vibration in the line of the radius, a vibration of some three millions of miles in extent, each phase of which occupies six months. This vibratory movement has its full effect upon the resultant motion of the earth, changing its orbit from a circle into an ellipse.

But the vertical vibration is resisted by gravitation in its outward phase, and aided in its inward phase. The result is that a portion of the motive energy of the earth is consumed, by the resistance of solar gravitation, during its outward movement. This lost mass motion must fall back into the earth and become a vibration of particles, constituting an increase of temperature. Its inward movement is, on the contrary, aided by gravitation. The mass motion increases at the expense of the temperature energy.

The loss of mass motion in the earth, from this cause, between perihelion and aphelion, is about 1¼ miles, or 6600 feet, per second. It will consequently not be difficult to obtain an idea of the amount of variation in temperature from this cause. For we know that a mass of water, when arrested after a fall of 772 feet, gains 1° F. in temperature from a conversion of its mass motion into heat vibration. Now a fall of 772 feet yields a final velocity of about 220 feet per second. If the loss of this velocity yields water a temperature of 1°, the loss of 6600 feet per second of velocity by the earth should yield it an increased temperature of 30° F., supposing its mean specific heat to equal that of water. If the specific heat equalled that of iron the increased temperature would be about 270°, and if equal to mercury it would be 900°.

We have here a very marked result, but one that is not strikingly evident, from the fact that this lost motion is not an instantaneous arrest, but a gradual arrest extending over six months. The true result, then, is daily increase in temperature, for every particle of water in the earth of one-sixth of a degree, of 1½° for every particle of iron, of 5 per cent for every particle of mercury, and a like result for every other substance in accordance with its specific heat. During the return movement of the earth, from aphelion to perihelion, the opposite effect results. Its mass motion increases, at the expense of its temperature, to an equal degree.

This variation in temperature cannot have any very evident effect at the surface of the earth, where it is lost in

the much greater effect of the solar radiations. But in the earth's interior it may possibly produce important results. The variation in the earth's internal temperature, through loss by conduction, is exceedingly minute. But we have here a source of a considerable increase during six months of the year, and a like decrease during the succeeding six months.

These daily variations cannot be lost by radiation, but must accumulate, so that the temperature of internal water must vary 30° yearly, of mercury 900°, and of other substances in like manner. Although we do not know what results are likely to arise from such an annual variation in temperature, yet it is very possible that these results may be of an important character.

In the case of a planet of short period and great simplicity of orbit, such as we have in the planet Mercury, the effects resulting from this cause must be much greater than in the earth. It, indeed, must produce a marked effect on the surface temperature of Mercury, and an annual variation sufficient to partly neutralise the variations in the amount of solar heat upon this planet.

An interesting conclusion from the hypothesis here advanced is in regard to the simple and natural method in which one mode of motion becomes converted into another. The change from heat vibration into mass motion needs no special machinery and no difficult transfer of energy. Motion seems to be constantly at the command of attraction. The least definite pull in any fixed direction, if unresisted by opposing energy, at once converts heat motion into mass motion. This latter, in its turn, is persistent until resisted, when it immediately becomes converted into the independent movement of particles.

The change from electricity to heat is probably as simple in its nature. The impelling cause, in all cases, seems to be some variation in attractive conditions, to which the moving particles instantly respond, their modes of motion becoming special results of the modes of attraction.

And as there is but one motion, so there is, in all probability, but one attraction. Gravitation, chemism, and magnetism are probably modes of attraction, as heat, electricity, and mass movement are modes of motion. The different forms which these assume very likely result from the different relations of position assumed by the particles of matter. It is probable, also, that molecules have special relations of position between their constituent parts, and that their outward attractions become specialised in

consequence. The relations of position between particles or masses at a distance from each other are general, and their attraction takes the generalised form of gravitation. The relations of position between particles in close contiguity are special, and their attractions become specialised. The modes of motion resulting are in direct response to the mode of attraction, and are readily convertible into each other at every variation in attraction.

As the generalised mode of attraction is gravitation, so the generalised mode of motion is the movement of the gas particle. This is so vigorous in its action as to resist the attracting energies of contiguous particles. Its motion is, therefore, influenced in vigour only through impact, and in direction only through impact and gravitative attraction. It is constantly falling in response to gravity, and constantly rebounding in response to impact. Wherever the resisting impacts are reduced in quantity the gas particles move in greater number, this movement constituting a wind, which increases in force as the resistance to the individual movements of the particles decreases in quantity.

Give the particles an opportunity to strike together with special ease in one direction, and a wind necessarily ensues. A fall, in response to gravitation, only ensues when the particles near the surface are separated by increased temperature, or through some other cause, so that their resistance to impact is decreased.

Attraction of gravitation, therefore, has no influence in increasing or decreasing the motive energy of matter. Its only influence is directive. It controls the direction of the motions of particles, so far as its control is not resisted by some other controlling attraction. The direction and mode of motion of the particle, at any instant, is a resultant of all the attractive and repulsive forces acting upon it at that instant, gravitation being simply a constant component of these forces.

The vigour of motion possessed by the particle can vary only in two ways. One of these is by impact, in which the energies of the two impacting particles may become changed, their sum remaining unchanged. The other is by the resistance of attraction. Here the particle loses motion, but gives its lost motion to the attracting particles, which it drags into swifter speed.

Motion cannot die nor be born. It can only be transferred in amount and changed in direction.

IV. THE AURORA.*

THIS is the day of *editions de luxe*. Even Science shares the decorative emotion, and appears in holiday attire. Of the many objects in Nature which attract alike the fancy of the artist and the more sober imagination of the scientific student, few possess the same kind of fascination as that afforded by the Northern Lights. Their mysterious and capricious uncertainty; their ever-changing aspect; their delicate *nuanees* of tint; the weirdness of the surrounding objects in those regions where they are seen in greatest perfection; and, lastly, the completeness with which they baffle explanation—all these constitute a set of attributes possessed by no other natural phenomenon, not even by the rainbow.

The volume recently brought out by Mr. J. Rand Capron, entitled "Auroræ: their Characters and Spectra," is indeed worthy of its attractive title. Well printed, possessed of spacious margins, and illustrated with several fine chromolithographs and many uncoloured plates, the work now lying before us presents an inviting contrast to the many cramped and ill-printed treatises which enshrine so many precious chapters in Science.

All that can be gleaned from past or present literature on the subject of the Aurora, Mr. Capron has embodied in his treatise. While giving us epitomised notes of the latest researches of Lemström, Backhouse, and R. H. Procter, he does not forget the good work of Sir J. Franklin and of Parry, and the older observations and speculations of pre-scientific ages obtain their due meed of notice. The pages of Seneca, Aristotle, and Josephus alike bear witness to the interest excited by auroral displays; and from the first-mentioned author the passage may be recalled in which it is narrated how, under Tiberius Cæsar, the cohorts ran together in aid of the colony of Ostia, supposing it to be in flames, "when the glowing of the sky lasted through the great part of the night, shining dimly like a vast and smoking fire."

Almost every observer of Auroræ has formed his own impressions as to the character and origin of the phenomenon. It is hardly strange therefore, considering the immense

* Auroræ: their Characters and Spectra. By J. RAND CAPRON, F.R.A.S. London: E. and F. N. Spon. 1879.

variety of appearances which the Aurora may assume, that the different accounts differ greatly. One observer is struck by the steady pale gleam of the arch of light; another by the ruddy streamers spread out fan-wise; a third by the drifting coruscations which seem to sweep at a prodigious rate across the sky. What wonder if the accounts are occasionally tinged with details derived unconsciously from the observer's imagination, though set down in all sincerity as objective facts. Mr. Capron has impartially given all these marvellous details supplied by different observers; and without pretending to sift their claims, or to reject, or criticise in any general manner, he lets each record stand upon its own merits. This method of treating the subject, while possessing many obvious advantages, has the one disadvantage of perplexing the reader, and compelling him to refer continually from the points in one set of observations to those in another. For many purposes the work would have been more valuable had something more been done towards guiding the reader towards a harmonious conception of the facts. But from the very nature of things this must be extremely difficult, and we should indeed be sorry to lose some of the precious details of isolated observations, which would run a risk of being shorn away in any such process of condensation. Isolated or exceptional facts, when really well established as facts, furnish indeed the most significant clues towards further phenomena: they are the finger-posts of scientific discovery. From amongst them we will select a few samples:—

On the 3rd of October, 1877, Herr Carl Bock witnessed an Aurora in Lapland sufficiently brilliant to enable him to sketch it in oil colours. A reduced facsimile of this painting is given opposite p. 25 of Mr. Capron's book. A significant point in Herr Bock's observation was that the auroral arch appeared to consist of two Auroræ, one behind the other, a quiescent arc in front, and a set of moving streamers behind.

Capt. McClintock makes the important observation that when Auroræ are present the atmosphere is never quite clear, there being usually a bank of low fog or cloud below the auroral streamers.

Prof. Lemström, in 1868, when accompanying the Swedish Polar Expedition, observed an auroral streak to burst forth suddenly during a fall of snow. The same author made some remarkable observations on the appearance of luminous beams around the tops of mountains, which the spectroscope proved to be of the same nature as the true Aurora.

Another precious and pregnant indication is to be found

on p. 38 of Mr. Capron's book, in the chapter on "Some Qualities of the Aurora," in which it is stated that Captain Parry with two companions saw a bright ray of the Aurora shoot down between him and the land, not more than two miles away. Accustomed as one is to regard the Aurora as a phenomenon of possibly the same magnitude as that of the zodiacal light surrounding the sun, this observation is of special and novel interest, particularly as it appears to be confirmed by similar phenomena noted by Sir W. Grove, Mr. Ladd, and Mr. Capron himself. The gleams of light witnessed by Sir W. Grove at Chester passed between himself and the houses, and, as they were continuous with the streamers above, "he seemed," as he says, "to be in the Aurora."

To obtain a knowledge of these and many other points of kindred interest, the student cannot possibly do better than consult Mr. Capron's exhaustive treatise: it teems with similar points; it states them with perfect clearness of style, and it furnishes the enquirer with an extended bibliography of the whole subject. Amongst the more remote topics will be found a carefully-compiled chapter on the sounds declared by many observers to accompany the exhibition of the auroral lights, but concerning which there is much conflict of opinion. Another chapter deals with the curious reddish patches of light occasionally seen upon the surface of the moon, which are suspected—and not without some show of reason—to be of a kindred nature with the Aurora.

The general reader will find matter for pleasant thought in the brilliant and exciting descriptions of auroral displays quoted from the narratives of Arctic travellers, particularly from that of Lieut. Weyprecht, which is graphic in the extreme. We extract from p. 28 the following account of the Aurora Australis seen by Capt. Howes, of the *Southern Cross*:—

"Our ship was off Cape Horn in a violent gale, plunging furiously into a heavy sea, flooding her decks, and sometimes burying her whole bows beneath the waves. The heavens were black as death, not a star was to be seen, when the brilliant spectacle first appeared. I cannot describe the awful grandeur of the scene; the heavens gradually changed from murky blackness till they became like vivid fire, reflecting a lurid glowing brilliancy over everything. The ocean appeared like a sea of vermilion lashed into fury by the storm; the waves dashing furiously over our side ever and anon rushed to leeward in crimson torrents. Our whole

ship—sails, spars, and all—seemed to partake of the same ruddy hues. They were as if lighted up by some terrible conflagration. Taking all together—the howling, shrieking storm; the noble ship plunging fearlessly beneath the crimson-crested waves; the furious squalls of hail, snow, and sleet, drifting over the vessel, and falling to leeward in ruddy showers; the mysterious balls of electric fire resting on our mast-heads, yard-arms, &c.; and, above all, the awful sublimity of the heavens, through which coruscations of auroral light would shoot in spiral streaks, and with meteoric brilliancy—there was presented a scene of grandeur surpassing the wildest dreams of fancy."

A very considerable portion of Mr. Capron's work, and of the illustrative plates, is devoted to the subject of the Spectrum of the Aurora as noted by different observers, and the various coincidences to be found between its bright lines and bands, and the lines and bands exhibited by the spectra of other known substances. Amongst more recent observers Lemström and Vogel are disposed to the conclusion that the spectrum of the Aurora agrees in the main with the spectrum of the air as illuminated by the electric spark. Angström concludes that the Aurora has two spectra, one of which gives the very bright greenish line always present in the Aurora, the other comprising the other and fainter lines. The very important work of Lord Lindsay, Backhouse, H. R. Procter, and Schuster in this department of research is also mentioned. An observation by Lecoq de Boisbaudran, cited *à propos* of the ruddy streaks so often seen in Auroræ, that the red line in the spectrum of phosphoretted hydrogen is increased in brilliancy by artificially cooling the flame, deserves careful attention. Summing up the evidence of the spectroscope as to the nature of the Aurora, Mr. Capron concludes as follows:—"As the general result of spectrum work on the Aurora up to the present time, we seem to have quite failed in finding any spectrum which, as to position, intensity, and general character of lines, well coincides with that of the Aurora. Indeed, we may say we do not find any spectrum so nearly allied to portions even of the Aurora spectrum, as to lead us to conclude that we have discovered the true nature of one spectrum of the Aurora (supposing it to comprise, as some consider, two or more). The whole subject may be characterised as still a scientific mystery, which, however, we may hope some future observers, armed with spectroscopes of large aperture and low dispersion, but with sufficient means of measurement of line positions, and possibly aided by photography, may help to solve."

On the general theory of the Aurora Mr. Capron is not very definite. He recounts a large number of experiments upon the action of powerful magnets upon the luminous discharges in vacuous tubes, undertaken with a view to elicit evidence on this question. His results show no important advance upon the researches of Plücker and De la Rive, and require, indeed, to be repeated by the light of the still more recent researches of Mr. Crookes, with which our readers are familiar.

Substantially Mr. Capron adopts Franklin's view, that the light of the Aurora is due to an electric discharge through the moistened air, the electricity concerned in the phenomenon being the product of evaporation in the region between the tropics, and passing at a great elevation into the Polar regions. This theory, it will be seen, almost precisely anticipates the theory of the Aurora put forward last autumn by Prof. Rowland. The theory of Lemström, and an account of the instrument devised by that physicist to illustrate the production of light by electric discharges as they pass into a stratum of rarefied air, are also given. Lemström takes Dalton's view, that the earth's magnetism acts in directing the position of the auroral discharge, but holds that the essential and unique cause of the formation of the light is the quiet discharge of positive electricity from the upper atmosphere to the earth—a discharge which has its counterpart in the lightning sparks of equatorial and temperate latitudes, but which differs from these simply by reason of the better conducting power of the colder and moister streams of air at the Poles.

Altogether the volume is, in spite of the drawback named above, a most valuable and admirable work, and we congratulate the author most sincerely on its timely appearance.

V. FLIGHTY ASPIRATIONS.

By FRED. W. BREAREY, Hon. Sec. to the A.S.G.B.

ADVENTITIOUS circumstances sometimes place men at the helm, who, being ignorant of all duties save one, assume the possession of all, upon the faith of having steered wisely in times past. So long as he retains the helm he will probably retain the confidence of his passengers; but let him act upon his assumption of knowledge in another sphere, and his ignorance may entail contempt. The human barnacles which fastened themselves upon the ship called "Progress"—sometimes the vehicle for the conveyance of such passengers as gas, lightning, steam on rail, &c.—have, it must be acknowledged, been nearly rubbed off, so rapidly has the "Progress" rushed through the waves of success.

So the helmsman has learned at last to stick to his tiller, and observe with respect, amongst other vessels, one freighted with such a cargo even as aërial navigation.

I have preserved an old barnacle. It will be found in the "Quarterly Review" for the year 1819. It was stuck to a ship that was being freighted with ideas for a railway:—

"We are not partisans of the fantastic projects relative to established institutions, and we cannot but laugh at an idea so impracticable as that of a road of iron upon which travel may be conducted by steam. Can anything be more utterly absurd or more laughable than a steam-propelled waggon, moving twice as fast as our mail coaches? It is much more possible to travel from Woolwich to the Arsenal by the aid of a Congreve rocket."

Don't you see that this barnacle was stuck upon a passing ship by the helmsman who quitted his tiller, and thereby manifested his intense ignorance.

This greatly dead and much-stained editor—as we may call him—may now be pictured laughing uproariously in presence of an enlightened audience, who look upon him with grave pity that so intelligent a man should be making such a humiliating exhibition of himself.

I am afraid that my remarks are not very respectful towards those editorial commentators who are apt to limit the aspirations of Science to their own conception of what is possible. What, then, can be said of a barnacle stuck on

with the authority of an acknowledged scientist such as Dr. Lardner, who, in his "Cyclopædia" of the edition of the year 1836, under the head of Hydrostatics,—which is too lengthy here to quote in full,—gives his reasons for asserting the impracticability of accomplishing, with any advantage, the then discussed employment of steam for ocean-going ships. He says—"But we have here supposed that the same means may be resorted to for propelling boats on a canal, and carriages on a railroad. It does not appear hitherto that this is practicable." He says again—"The friction of a carriage on a railroad moving 60 miles an hour would not be greater than if it moved but 1 mile an hour (!); while the resistance on a river or canal, were such a motion possible, would be multiplied 3600 times." By friction he means resistance, because in another place he says—"The resistance on the road, instead of increasing, as in the canal, in a faster proportion than the velocity, does not increase at all." So that we have it, upon the dictum of Dr. Lardner, that a wind blowing upon a surface at 60 miles an hour—the conditions are only reversed—produces no greater pressure than if it were blowing with a velocity of but 1 mile an hour. In each assertion of the rate of resistance Dr. Lardner was intensely wrong.*

The late Sir Wm. Fairbairn became a member of the Council of the Aëronautical Society of Great Britain. Now the supporters of this Society entertain two diverse opinions, but both parties aim at macadamising the aërial highway so as to make it subserve the purposes of transit. To the ordinary observer there is only one way. It is that which has been brought hitherto under his observation. Sir William was a balloonist. The balloon is a fact beyond dispute, so that all we have got to do, says he, is to propel it. It is given only to the man who has made its propulsion a study, and has been left gazing regretfully after the money which he has expended in his vain attempts after utility, to estimate rightly the opinions of that section which may be designated by the title "Gravitites," in opposition to that of "Levitites." The Gravitites contend that the object of aërial transit will be effected by opposing the resistance of the air to the action of gravity; that whilst gravity is a constant force the resistance of the air is under control, so that it can be made subservient to the support of any weight, the surface of which is sufficiently extended, and propelled with the requisite

* A train of carriages and engine weighing 300 tons would meet with a resistance of 3870 lbs., at 10 miles an hour, which would be increased to 12,470 pounds at 60 miles an hour, irrespective of its advance against the air.

velocity. For instance, a sheet of stiff cardboard can be propelled horizontally by means of a finger loosely placed at the rear edge. By increasing the velocity beyond the necessary requirement the cardboard, or any plane surface, will depart from the horizontal in an upward direction, in obedience to the increased resistance of the air, and the rate of velocity being increased it will turn over towards the hand.

It is the desire of some workers to obtain support in the air by extending the area of such surface and propelling by screws: upon a small scale this has been proved practicable. Models of dimensions and weight capable of being launched from the hand are very effective; but when those of a larger size, which cannot be thus manipulated, are attempted to be put to practical use, a preliminary run upon the ground is necessary, and hitherto the velocity under those conditions—being retarded by friction, although upon wheels—has not been attainable. This velocity is an absolute condition, so as to enable the apparatus to meet with that atmospheric resistance which would force it to leave the ground and continue its flight in the air. Certainly no rails have yet been laid down with the object of reducing friction, but the aid of an incline has been enlisted without effect. No experiment worthy of the object sought to be attained has yet been attempted by any one holding the opinion that eventual success lies in this direction.

My idea of a satisfactory trial would be the employment of great power, large and strong surface, and as frictionless a road as could be devised; for instance, upon a straight line of rail.

The interest which is attached to many scientific subjects is, however, absent in this, so far as respects the public and amongst scientific men generally. So little understood are the principles upon which the hope of flight is founded that it is well known that if a discussion is started in any scientific periodical there are scarcely any instructed minds to follow it up, and the subject dies away almost from its birth, eliciting nothing but worn-out ideas, and always drawing out the suggestion that gas should be used to take off the dead weight. This suggestion is as absurd as the converse one of using an aërial machine to propel a balloon.

Those who saw poor De Groof when he left Cremorne Gardens, in the hour of his death, dangling from the balloon in his comparatively fragile framework, will call to mind the diminutive appearance of the apparatus compared with the bulk of the balloon. To take off the dead weight would require as large a balloon as usual, but still of such a

capacity as would dwarf any attached apparatus, and it is quite certain that if the apparatus had any power over the balloon it would not be exerted to propel it, but to drag it at the stern.

So the earnest workers and students are a very small minority. For want of guidance and the dissemination of fundamental facts, the result of experiment, many have been working in the dark, and doubtless, encouraged by the general ignorance, many pompous announcements have been made during the present century which have raised false hopes, and the reaction has had a most injurious effect upon the study of Aërology with a view to the sustentation of heavy bodies. The fact is that a triumph over the difficulties of aërial transport presents to the mind which can grasp the future such an Aladdin-lamp romance that the individual is inclined at once to self depreciation, and to say that "not for me is such a fate in store."

Some such effect has operated to produce apathy as is recorded by Stewart in his "History of the Early Days of the Steam-Engine," as follows:—"Every miscarriage thus added to the obstacles which at all times impeded the introduction of improvements, and the abortive attempts of ignorant and designing men were urged as reasons for disregarding the inventions of more honourable and meritorious individuals."

I cannot leave the subject of plane propulsion without reference to a late attempt by Mr. Lenfield, of Winchester, whose design was suspended from the skylight of the large room at the Society of Arts, at a general meeting of the Aëronautical Society of Great Britain, in 1879. This formidable-looking affair was 40 feet long by 18 feet wide, attached to a framework upon four wheels, the whole rising about 15 feet: 300 feet of canvas was stretched upon an upper frame, and below this, and upon the wheel-supported platform, the operator stood with his feet upon treadles, by which he worked two fan-blades, 9 ft. 6 ins. × 2 ft. 9 ins., in front of the apparatus. By this he obtained about seventy-five revolutions a minute, which enabled him, upon a macadamised road, to attain a speed of about 12 miles an hour,—totally insufficient, however, to enable him to obtain any fulcrum upon the air, for its weight including himself was 304 lbs. Nor was a subsequent attempt down an incline, by which he gained a speed of about 20 miles an hour, any more suggestive of aërial support.

In order to give some idea of the solid support which a body of air is calculated to afford to any surface passing

over it in a horizontal direction, and which can be increased or diminished according to the angle in which it is propelled, I will quote some suggestive remarks of Mr. Glaisher, made at one of the Aëronautical Society's meetings, upon the subject of the captive balloon then lately exhibited at Chelsea by Mr. Giffard :—"That balloon has spoken to me trumpet-tongued. All my life I have been accustomed to weigh the air by grains. In winter I find the cubic foot to be about 570 grains, and in summer 20 or 30 grains less. When we took grains we thought the air light. When I saw the other day that the balloon would lift 16 tons or more, and consequently that the weight of air displaced by the balloon must be of greater weight, there must, I think, be something for members of the Society to work upon. When you see that balloon as a small ball only, yet know that air to so many tons weight was displaced by it, surely it held out the hope that some means would be found to solve the problem of aërial navigation."

The method to be adopted to attack this thin though weighty medium, so as to wrest from it the means of support in safety, and the mode of propulsion, is of course the subject of discussion and of some difference of opinion amongst experimenters.

Sir Wm. Fairbairn stuck another barnacle on the good ship "Progress" when he stated as his opinion, in a paper read at Stafford House, that "Man was never meant to fly; that if the Almighty had intended him to do so He would have given him wings, and that the unalterable laws of Nature were against us." Now it is still a disputed point whether a man possesses the power to manipulate anything in the nature of wings, so as to afford him support and propulsion. The few experiments which have been made in this direction are not sufficiently authenticated for us to deduce any reliable data from them.

Without wishing to dogmatise, and especially without laughing like the writer in the "Quarterly Review" before referred to, I hold, with the Duke of Argyll, the opinion expressed in his own words when occupying the chair at one of our meetings :—"I think it quite certain that if the air is ever to be navigated it will not be by individual men flying; but it is quite possible vessels may be invented which will carry a number of men, and the motive force of which will not be muscular action." I limit the application of these words to the action of wings by man's muscular efforts. I wish I could think that the late Sir Wm. Fairbairn had made the same reservation.

The laws of Nature, fortunately for us, are unalterable, and as often as they have been questioned as to their adaptability for aërial suppport they have returned a favourable reply.

It may be conceded that a properly constructed plane surface, propelled against the air, will meet with sufficient resistance to enable its course to be deflected upwards at an incline obedient to the angle at which such plane is driven, and sooner or later according to velocity.

An experiment is recorded in one of our annual reports which was made by M. de Louvrie. To a little carriage he fixed a thin plane surface, the angle of which he could alter at will. Placing this machine upon a level spot, he drew it along horizontally by means of a cord which was fastened to a dynamometer, and increased the speed until the machine left the ground, suspended by the pressure of the air on the plane.

In a large machine, such as Mr. Linfield's, where the balance cannot be adjusted with that facility which is readily attained with models launched from the hand, the difficulty will commence with the first tendency to rise. Supposing him to be unable to attain sufficient velocity with a new arrangement which he is constructing, his next step I presume would be to attach it to an engine on a railway, and repeat upon the largest scale the experiment just recorded. Now in such cases the laws of Nature are greatly in our favour, as proved by some experiments initiated by the Society to which I have the honour to act as Honorary Secretary. It remains as a condition—hitherto unfulfilled—that man must attain to perfection in his appliances before he can evoke the utmost effect which is capable of being wrested from Nature in her passive mood.

The experiments consisted in forcing a blast of air against various extents of surface presented to it at varying angles, in order to ascertain not only the force with which they would be driven back, but the weights which they would be able to lift by the air passing beneath the under surface of the various inclines.

Like the sheet of stiff cardboard propelled by the slight pressure of the finger against the posterior edge, supported by the pressure of the air underneath, it was required to know what that pressure was which tended to lift or tilt it up, because that knowledge would enable us to ascertain what weight it would bear to keep it from so tilting up, and also what amount of pressure forward, represented by the finger, would be required to propel it. And this is one of

the results discovered, viz., that propelled against still air, at an angle of 15°, and at the rate of 25 miles an hour, a square foot of stiff plane surface—in this case it was steel plate—will support a weight of 1½ lbs., whilst the resistance to its forward motion is only 5¼ ounces; so that it requires but little more than 5¼ ounces to propel it.

I must say that if we apply this calculation—the result of accurate experiment—to Mr. Linfield's arrangement of area, we do so to its great disadvantage, because he never contemplated advancing against the air at such an angle as 15°; and unfortunately, by some extraordinary oversight, the instrument employed in the experiment, and made expressly with that object, was unable to record any angle less than 15°.

Those very angles which most concern aërial experimenters were left out of the question. It may, however, readily be conceived that at less angles the resistance to the forward motion is less, consequently the power required to propel is less, except that greater velocity is required to keep the same weight in suspension.

But we will take the calculations at 25 miles an hour and 15°, and 300 square feet of plane surface. At that velocity a rigid plane surface would support, at the least, 450 lbs.: in reality much more than that, because the supporting effect of a plane increases in some yet undetermined ratio for each additional square foot. This, however, is not a rigid surface, and therefore some element of uncertainty exists.

However, it appears to be necessary for success to be able to propel such a surface at the rate of 25 miles an hour. We have it reported that down an incline Mr. Linfield succeeded in obtaining 20 miles an hour; but inasmuch as upon the level ground he did not travel more than 12 miles an hour, with the greatest number of revolutions which it was possible for him to impart by his muscular efforts to the screw-propeller, it is evident that the excess down the incline was due only to gravity. The additional air-pressure must have had the same effect upon his screw-blades as the wind produces upon the sails of a windmill, thereby accelerating the speed of the screw without producing an increase of propulsive force, because the air would pass with a greater velocity than that at which the screw was working, rendering it impossible for him to keep his feet upon the treadles. The aid of an engine and railway would therefore be no assistance to anyone trying a similar experiment. It is, I think, conclusive that man has

not sufficient power to revolve a screw capable of giving him the necessary speed to leave the ground. Whether if he left the ground, and travelled only in the frictionless air, he would under such conditions possess the power to continue his flight, remains undetermined. He could only be introduced into such a position for determining the truth by first being flown as a kite, without any screw action, and then, having attained that position, he could set to work with his screw. The question of balance is a most important one for consideration. I have known repeated attempts to effect the flight of models entirely fail for want of a proper adjustment of weight. The attainment of that necessary condition for success would be not the least of the difficulties to be encountered by the plane propeller, where he himself is the adjustable weight, yet confined to his work at the treadle.

The conclusion is I think inevitable, that another and more powerful motor than that of a man is necessary to get the initial velocity. I think that it will have occurred to the reader of this paper that the screw-blades themselves are a source of hindrance to progression; that whilst al else lies along the plane of progression, the screw-blades offer a direct opposition to the air the moment that the operator travels by any extraneous aid *faster than he can revolve the screw.*

Here is unmistakably shown the enormous advantage of the wing-movement, where the means, both of support and propulsion, lie in the plane of progression, and only edge-resistance is offered to the air. The notion of wings, however, is nearly always ridiculed. Somehow we are inclined to contemplate them as we are accustomed to see them pourtrayed upon the backs of angels, where the muscular development is singularly wanting; and looking at a full-grown angel, as depicted by some of our artists, one does really doubt his ability to fly.

Had Sir Wm. Fairbairn such an imbecile in his mind? Possibly, as those who entertain such flighty aspirations are supposed to be devoid of common sense, it might be supposed that we relied upon these angelic prototypes as our authority. Well, I will at once disabuse their minds. We object to them strongly, not only because the proportions and malformation are unworthy of the object, but because they have n't any tails!

That, however, which painfully strikes me as wanting in any plane surface used merely as a fixed plane, is the apparent absence of stability. I should not care to trust myse

to the possible vagaries of a screw-propelled plane. It seems to me to be a structure without any life in it, unable, as it were, to help itself. It represents the fag end of a bird's flight when he rests from his labours, and, by the aid of the impetus gained, continues his journey with motionless wings extended as a plane.

This plane he can also use as a propeller; and therein consists one of the grand instances of superiority over any other mode of aërial transit. But it is the manner of its use which demands from us the acknowledgment of its great advantage.

The action of a bird's wings is exerted in a space of *three dimensions*, measured by the length of the wings from tip to tip—the arc of vibration of wings—and the length of the body, to which might perhaps be added the effect, both in front and rear, of a reaction arising from their attack upon the air. The stability which by this process is attained, within the knowledge of every one, exists in spite of the fact that the wings are moved up and down, although alternately above and below the centre of gravity of the bird, and although the head and the tail are really acting as the scales of a balance of which the wings are the centre. Those who have watched the flight of wild ducks must have been struck with the peculiar contour which they present. The wings appear to be the cross centre of a cylindrical shaft. The dimensions of a tame duck which I have just measured are—Extreme length from toe to beak, 29¾ inches; from beak to root of wing, 14 inches; stretch of wing from tip to tip, 32 inches; so that the wings are only 2¼ inches longer than the whole body, which in flight would present this shape:

In all probability this bird flies in a stratum of air—taking 1 foot as the extent of vibration of wing—more than 6 cubic feet of which is put into a state of commotion in every sense conducive to its support and balance.

Whatever may be thought, therefore, as to the folly of obtaining flight by wing-vibration, nevertheless it is to that, or to some application of the principle, that we must come in our attempt to make flight serviceable for man.

It is very doubtful whether man has the power necessary for the manipulation of wings of dimensions and strength

sufficient to afford him support, but I think it very possible and probable that he may construct an apparatus with surface sufficiently large to sustain him and the additional weight of a motor powerful enough to attack the air so as to obtain support and proportions from it.

We know that the long-winged bird, such as an albatross, has a very flexible wing, which hangs quite droopingly from the body, and which being vibrated produces a wave-action from the root of the wing to the tip.

It is not the lot of many who will read this paper to shake carpets, but to all it will be understood how the wave of air compressed in the downward shake is propelled underneath, so as to throw the carpet into waves, and how—if with sufficient power—those waves are bound to escape at the opposite side; and if with rapidity of wave-action, how the carpet may be made to hover above the floor without contact. Suppose that we can communicate such an undulation to a fabric free to vibrate in the air; then its progress would be exactly opposite to the direction of the wave, and it would entirely depend upon the power that we could employ to enable us to determine what weight of fabric could be thus thrown into wave-action, and thereby supported and propelled; or, in other words, how light a fabric could be used, and what additional weight could be substituted. Had Mr. Linfield's arrangement of 300 square feet of fabric been provided with the power to impart this wave-action, a very different result might have been recorded.

All the experiments which I have made in this direction, with models set free in the air, have pointed to probable success when manufactured upon a scale of utility. This means the capability of sustaining and propelling 1 lb. of weight for every square foot of surface, and this capacity ought to be sufficient to provide for the sustentation of the necessary motive power. The action of such an apparatus may be thus described:—A kite-like structure, the two arms of which are not fixed like the kite, but are free to move, in the manner of the wings of a bird, with a sweep of perhaps 6 feet; width of such arms from tip to tip, 20 feet; the fabric attached to the arms, and extended backwards some 30 feet, affording with its triangular shape above 300 square feet; from thence a tail, capable of elevation or depression; the whole capable, by the aid of steam, of acting upon a stratum of air of perhaps 3000 cubic feet.

It may be disputed—as it has been—that a fabric shaken as described has any propelling tendency, because, say the

objectors, "If you leave your hold it will recede from you." My prescription would be "Keep it well shaken." But although I have advanced this illustration of the carpet, the cases are not exactly parallel. By the peculiar vibration given to my arrangement a double wave-action is imparted, —that is, along the wing-arms, and obliquely throughout the fabric from front to back,—so that the air is propelled laterally, the fabric being attached to the body or shaft which runs down the centre like the back-bone of a kite.

For all answer the propelling power of such an arrangement has been proved to be effective. The sustaining property is not disputed. Moreover, owing to the "bellying" of a large surface, from the great resistance offered by the air, a gradual and safe descent is ensured in the direction of advance, upon cessation of the motive power.

This is my "Flighty Aspiration," irrespective of details, and I contemplate its construction.

ANALYSES OF BOOKS.

The Theory of Colour in its Relation to Art and Art-Industry. By Dr. WILHELM VON BEZOLD, Professor of Physics at the Royal Polytechnic School of Munich, and Member of the Royal Bavarian Academy of Sciences. Translated from the German by S. R. KOEHLER, with an Introduction and Notes by E. C. PICKERING. Boston: L. Prang and Co. London: Trübner and Co.

WE have here a thorough and masterly exposition of the laws of colour as applied to industrial and artistic purposes. The author has most important lessons to convey, not merely to the painter, but to the tissue printer, the dyer, the paper-stainer, the upholsterer, the manufacturer of glass and porcelain, the worker in mosaic, and the jeweller, all of whom will find here valuable principles laid down for their guidance. Natural good taste and what is called a quick eye for colour will doubtless do much, but without distinct and systematic knowledge they will frequently fail to save their predecessor going astray. It may even be suggested that the propensity for sombre, dull, and impure colouration in modern costumes is to be traced to ignorance. Fearful of the wretched effects admittedly produced by the improper combination of pure colours, we take refuge in a general murkiness, which at any rate makes our failures less conspicuous. A work like the present may therefore justly claim a very high industrial, artistic, and social-æsthetic importance.

The author naturally opens with an examination of the physical basis of the theory of colour, involving, of course, a description of the spectrum. Here he explains very clearly that great stumbling-block to numbers of even well educated persons—the essentially subjective character of colour. He also takes an early opportunity of pointing out the distinction which exists between colours and pigments. If we mix together a yellow and a blue pigment,—say gamboge and ultramarine,—or if we dye a piece of silk with indigo and then top it with weld, we obtain, as all the world knows, a green. On the faith of this and similar experiments it was decided that green is a compound colour, and that the only simple primary colours are yellow, red, and blue. The reason, however, why green is produced by the mixture of a yellow and a blue pigment is not difficult to trace. The light passing through them undergoes what may well be called a process of subtraction. Suppose we pass light through Prussian

blue and through gamboge, and allow it to fall upon a prism. We shall find the spectra thus obtained very different from that of white light. In that of gamboge, the violet, ultramarine, and a portion of the blue are cut off. In the spectrum of the Prussian blue, the red, orange, and yellow are cut off. Now every ray of light that falls upon a mixture of these two pigments undergoes this double process in the particles from which it is transmitted and reflected, and the result of the subtraction process is that green remains. But if we throw the blue portion of one spectrum of white light upon the yellow portion of another, we have a process of addition, not subtraction, and the result is a grayish white. From modern researches it appears that if we leave pigments out of the question, and attend to colours in the simple sense of the word, the three fundamentals are red, green, and a blue-violet.

The author's classification of colours is different from the celebrated circles of Prof. Chevreul. His arrangement is represented by a cone, on the base of which are arranged in a circle the following colours:—Carmine, vermillion, orange, yellow, yellowish green, green, bluish green, turquoise, ultramarine, bluish violet, purplish violet, and purple. All these colours, it will be observed, are prismatic except "purple." This name the author gives to a colour which is not found in the spectrum, but which would form the intermediate gradations between carmine and violet. It coincides tolerably with that of the dye-ware commonly known as magenta, but technically called rosanilin acetate. If we then place white in the centre of this circle, all the gradations between it and the above twelve colours can be arranged in lines running from the centre to the circumference. Here therefore will be the places of such colours as rose, pink, peach, lilac, and lavender, which are respectively dilutions of carmine and of the various grades of purple and violet.

Again, at the apex of the cone we place black. On lines ranging along the exterior surface of the cone, from the base to the apex, are arranged such colours as olive (a green saddened with black), the browns (which according to their various nature may be either yellowish green, yellow, or red saddened in a similar manner), wine-colour (which may be called a blackened purple), &c. The true greys—*i.e.*, the gradations from white to black—fall along the line passing from the centre of the base to the apex.

In the fourth chapter the author deals with the theory of contrast—a subject based almost entirely upon physiological considerations. A number of easy and familiar experiments prove that any coloured object, if regarded fixedly for some little time, causes the observer, on turning away, to see its image, but changed to the complementary colour. Thus after looking at a red wafer placed upon a sheet of white or grey paper, we fancy we see a bluish green circle of a similar size to the wafer. But

contrast is not merely successive, but also simultaneous. The author formulates the following general law:—"If by the side of a given colour we place any other colour, the first will apparently be changed, as if some of the complementary of the second colour had been mixed with it."

In the fifth chapter Prof. von Bezold enters upon the practical application of the preceding considerations,—the uses and combinations of colour in the decorative arts and in painting. Here he refutes the theory of Field, reproduced by Owen Jones, that the most favourable impression is obtained by employing colours in quantities so proportioned that their mixture would produce pure grey. He shows that a pattern of warm colours upon a cold ground produces a better effect than the opposite arrangement. The number of useful hints scattered through the book must, we feel sure, both surprise and please the practical man, who will here see sound reasons for much that he has hitherto done instinctively or arrived at by a process of groping. He will see why yellow, yellowish green, and turquoise never produce a happy effect upon velvet, a material which is best adapted for violets, purples, reds, and greens; in a word, for full colours. Where the surface reflection is great, and where much white is necessarily mixed with the colours, the very reverse is the case. Violets on satin are apt to take a faded or washed-out appearance. Turning to a totally different region we find the reason why the inside of a golden cup appears of a deeper and richer colour than the outside,—*i.e.*, because the inside is, by reflection, illuminated with yellow light. Hence the inside of silver vessels is very commonly gilt, but the outside rarely.

An important feature of the work consists of its beautifully coloured illustrations, which enable the reader to make observations, and we may almost say experiments, confirmatory of the doctrines taught in the text. For instance, we have certain words, in a bold heavy type, printed with ordinary black ink upon a green paper. If we look at them through the appended leaf of thin tissue paper the black letters become purple.

We consider that this work can scarcely be over-estimated; and whilst regretting that our examination has necessarily been brief, we recommend it most warmly to all who are concerned with the use of colours.

The language of the translator is not incapable of improvement. Thus, p. 188, we read that "mouldings which ought to be horizontal *may be given* a slight inclination." We were not aware that this modern solecism had spread to America.

The Constitution of the Earth. Being an Interpretation of the Laws of God in Nature, by which the Earth and its Organic Life have been derived from the Sun by a Progressive Development. By ROBERT WARD. London: George Bell and Sons.

WE have here an exceedingly interesting work, which, though by no means free from errors, places before us bold and startling conclusions, and thus prompts its readers to research. The author propounds the following four laws:—

1. Circumstances govern the creation of things, and therefore all things exist by virtue of their circumstances.
2. As no two things can occupy the same place in the universe, therefore no two things can be exactly alike.
3. The differences or similitudes between two things must be in proportion to the differences or similitudes in the circumstances under which they have come into existence, or by which they are sustained.
4. A change in the circumstances of things necessarily involves a corresponding change in the things themselves.

By means of these laws, applied to a variety of phenomena, the author arrives at conclusions strangely at variance with the teachings of our received scientific text-books. He holds that the earth and all the planets have been derived from the sun, and in like manner the satellites from their primaries. Each planet in the course of ages gradually recedes from the sun, increasing at the same time in size. The rotatory motion of each increases in a ratio having a relation to the distance. As the planets grow older and become more developed, they become more independent of the sun for both light and heat. "We are justified in assuming that magnificent worlds, like Jupiter, are the abodes of life of corresponding importance, and that their quicker motions in some way compensate for the weaker rays of the sun, which they will ultimately dispense with altogether." The southern portion of the globe, which is more immediately presented to the sun when the earth is nearest thereto, is in an inferior state of development to the northern. The moon has no rotatory movement; it is a child of the earth, created long ages after the earth, and its want of air and water are not marks of decay, but of a development as yet imperfect. Our world, instead of being united to the sun, "is destined to a glorious future, in which it will expand in magnitude, increase in created splendour, evolve new properties, new laws, new elements, add satellite within satellite to its train, and ultimately become the centre of a system as magnificent as the sun's!" When the earth was no older than the moon now is, it would be in a similarly undeveloped condition.

The author denies the circulation of matter. He contends that there is no evidence showing that the exact amount of water which falls from the atmosphere is again returned to the atmosphere, and that the ether in which the earth moves affords an obvious and boundless material for the creation of clouds.

We must here interrupt our exposition of the author's views. When we have full proof, as Mr. Ward admits, that portions of water exposed to the air disappear by evaporation, it seems to us unwarrantable to call in another source for clouds and rainfall of which we have no proof whatever. The experiments of Dr. Wells do not demonstrate that the dew-forming vapours have any other origin than the moisture exhaled from the earth.

The author then contends that entire oceans of water have been converted into solid rock, "in opposition to this theory of the mechanical circulation of matter." The lime, *e.g.*, of which coral-reefs are formed he supposes not to be matter which the water had dissolved out of the crust of the earth, and which the coral animalcules merely separate out in a solid form, but to be generated by the water. Thus "unless replenished by condensations from the ether, the entire ocean seems destined to be transformed into stone." Water, we are told, begins in vapour and ends in salt. The saltness of the ocean is the beginning of its solidification. But where is the actual evidence that pure water, preserved in vessels which it cannot dissolve, will ever become anything but water, or that we can obtain from it anything save the elements of water—oxygen and hydrogen? Let Mr. Ward obtain from water one grain of salt or lime which is not derived from some extraneous source, and he will at the first vacancy be elected President of the Royal Society. Salt is the only mineral body which we eat as such; but does it therefore follow that the lime, phosphorus, iron, sulphur, &c., found in our bodies are due to its transformations? All these substances can be detected in our food.

But we must hasten on, for Mr. Ward has yet much to tell us. The chinks and fissures in the earth's surface, the ravines, canons, and river valleys are the results of the earth's expansion. Her outer layers crack like the bark of a growing tree. As regards the hypothetical growth of the earth, the author's views coincide with those expressed by Capt. A. W. Drayson, R.A., in a work published as far back as 1859, under the title "The Earth we Inhabit." This writer states that "whilst the most perfect accuracy was supposed to have been attained in Astronomy and Surveying, still, when the results obtained by the two sciences were compared, the most alarming differences were almost invariably found to exist. The more perfect the instruments and the more skilful the observer, the more surely was a difference found." Both Capt. Drayson and Mr. Ward ask, essentially, "Is it a fact that, whenever distances have been carefully measured after the lapse of any considerable number of years, differences have always

been found to exist, and in every instance the later measurement has shown a greater number of feet and inches than the former?" It may here be mentioned that some years ago we were informed by a solicitor who had a very large practice in the transfer of real property that he had always found the size of an estate, as given in old title-deeds, fall most decidedly short of the results as ascertained by a recent survey. He ascribed this discrepancy to the inaccuracy of the old measurements, though it must be conceded as strange that the error should always fall in the same direction. It is asserted, in Capt. Drayson's work, that since 1831 the equatorial diameter of the earth has grown 5574 feet and the polar diameter 3180 feet. The latitude of Edinburgh Observatory, as given in 1827 by Francis Baily, then President of the Astronomical Society, when compared with 1858, shows an apparent movement of 1373 yards. The Observatory of Berlin in 1845, as compared with 1858, differs to the extent of a mile and a hundred yards. The general public, and even men of science whose speciality lies elsewhere, must stand aghast at such statements, and be at a loss which is the less incredible supposition—that eminent astronomers should be in error on such a point as the latitude of an observatory, or that buildings should be capable of undergoing a movement of translation, amounting, as in the case of Edinburgh, to 40 yards per year.

It will be remembered that the distance of the earth from the sun, as deduced in 1874 from the observations made on the transit of Venus, was found greater than had been previously calculated. This Mr. Proctor refers to the untrustworthy nature of Delisle's method, whilst our author takes it as a confirmation of one point in his theory—the recession of the earth from the sun.

It must be admitted that the work contains not a few errors in points of fact. Thus we are told that aniline is the base of all the colouring-matters obtained from coal-tar; and again that picric acid, peonine, azuline, &c., are aniline-colours; that lithium is ranged among the non-metallic bodies, &c. We find also reasoning which to us appears strangely inconclusive. The modern doctrine that colour is nothing inherent in bodies themselves, but is merely a form of animal consciousness, is declared "open to the somewhat startling objection that if true it would follow that there would be no rainbow if there were no human beings." To us this seems no objection at all. Still the author's views, if novel, are tangible; they admit of being verified or refuted by observation and experiment, and to these he appeals. Hence we must consider his work deserving of serious attention.

As a defect, we feel compelled to notice the number of typographical errors. Thus we read of albumious, oeline, naphthalian, corana, &c. A modern work is quoted as "Kingzett on the Alki Trade."

Six Lectures on Physical Geography. By the Rev. S. HAUGHTON, M.D., D.C.L., F.R.S., &c. Dublin: Hodges, Foster, and Figgis. London: Longmans, Green, and Co.

THE work before us is one which for many reasons deserves our most hearty welcome. It is an exposition of some of the latest results of modern science, and though popular, in the sense of being intelligible to the educated lay public, it is not the less accurate, comprehensive, and thoughtful. To many well-meaning but timorous souls the author's attitude with reference to certain burning questions will prove not a little salutary. Dr. Haughton is no rash and shallow theorist, but a writer whose sound learning and sobriety of judgment are beyond dispute. He is, further, a believer in a personal God, and in the authority of the Scriptures as a moral and spiritual revelation. But he finds no difficulty, on this account, in accepting the doctrine of Evolution, whether as regards the heavenly bodies or their organic inhabitants, and in admitting a minimum of two hundred million years for the past life-time of our planet.

Among the many interesting subjects dealt with in this volume we will first glance at one which has often been discussed in the "Journal of Science,"—to wit, the Glacial epoch. Dr. Haughton rejects "with contempt" the theories which refer this dreadful visitation to a change in the position of the earth's axis of rotation. He also rejects all solutions based upon the secular cooling of the earth. It is of course obvious that such a gradual cooling down, if it might serve to explain the advent of the Glacial epoch, could not possibly account for the return of a better temperature. The changes in geological climate he refers to the gradual cooling of the sun, involving a corresponding refrigeration of the earth's surface. Glaciation he thinks due to a temporarily diminished rate of heat-radiation from the sun. A natural consequence of such a change would be the precipitation of the aqueous vapour suspended in the atmosphere. Now as this same watery vapour is one of the main agencies which prevent the earth from radiating into space most of the heat which it receives from the sun, and which thus moderate the cold of the night and of the winter, it follows that such a precipitation would be "followed by an increased radiation of non-luminous heat into space from the earth's surface." Now, though we have no positive proof of a transient decline in the sun's heating power during the Glacial epoch, we still know that bodies like our sun do not decrease in radiance at one uniform rate. They have been observed to fade down, and again to blaze forth with increased splendour. In supposing that the sun has once, or more than once, been thus dimmed, we merely assume in him such changes as have occurred elsewhere in the universe. As a necessary

consequence, if glaciation was due to a temporary decrease in the sun's radiant heat, it must have occurred simultaneously in both hemispheres, as was concluded by our late friend Mr. Belt, from his geological researches. It need scarcely be said, however, that Dr. Haughton utterly disagrees with Mr. Belt as concerning the cause of the Ice Age. Our author, in passing, notes the absence of all evidence of glaciation in Siberia. Whether this absence extends to all parts of that vast region, and whether future research may not somewhat modify our conclusions in this respect, are perhaps open questions. But what if, during the Glacial epoch, north-eastern Asia was mainly under water? It seems, even since what may be called historical times, to have been undergoing a gradual elevation.

As factors in the climatic peculiarities of earlier geological ages, Dr. Haughton calls attention to two points often overlooked, viz., the large quantity of carbonic acid in the atmosphere during Palæozoic times, and the excess of watery vapour in the Miocene period. Both these bodies, especially the former, have a much greater power than oxygen and nitrogen in arresting the passage of non-luminous heat. It is scarcely too much to say that if the atmosphere were equal in this respect to carbonic acid, frost would be unknown outside the Polar regions.

As regards the origin of life upon our globe Dr. Haughton rejects, with well-merited ridicule, the wild hypothesis which seeks to derive organic existence from an aërolite,—the "moss-covered fragment" of some disrupted world. As we pointed out on a former occasion, this assumption merely postpones the difficulty,—"lengthens out the disease," like Falstaff's loans, unless we can explain how life took its rise in that other world. Our author sees nothing unphilosophical in the assumption that organisms may have been produced from lifeless matter, under peculiar, Divinely-arranged, conditions.

Dr. Haughton's views on the future of our globe do not agree with the expectations of poets and orators. He shows that ultimately Mother Earth will become an airless and waterless orb, revolving on its own axis in the same length of time as she revolves round the sun, one hemisphere being scorched with intolerable heat and the other pinched with unceasing frost,—a counterpart, in fact, of the moon. Indeed the author is not, in the strict sense of the term, a Uniformitarian. He contends—and what geologist can contradict him?—that the earth has already experienced many marked changes of climate. But such changes will scarcely be denied by the ordinary Uniformitarian, who simply holds, in opposition to the Catastrophist, that the organic population of the globe has not been completely extirpated some eight or ten times, being then, after a "period of repose," succeeded by a fresh creation. We find in the work before us no evidence to connect Dr. Haughton with this latter school.

To the following passage in the opening Lecture some exception may perhaps be taken. The author says:—"There have been from the earliest times two classes of speculation on this subject,—two distinct poles or forms of thought. In the one case we have a race of people distinctly imagining to themselves a personal Creator of themselves and of the universe; a tone of thought leading to an elevation of man above surrounding things, and to a contempt for Nature. In the other case we have a belief in blind formative forces animating matter, involving with it a contempt for man as a miserable unit, as the sport of circumstances, and a corresponding high view of the dignity of Nature." We must confess ourselves unable to see why the conception of a personal Creator should lead to a "contempt for Nature," which is His work. Nay, the entire antithesis between Man and Nature seems to us not less vicious than the old contrast between the earth and the heavenly bodies,—which now survives merely as a rhetorical flourish.

In conclusion we most warmly recommend these Lectures to our readers, and we salute the author as one who, like ourselves, recognises in Evolution God's method of creation.

The Art of Bookbinding. By JOSEPH W. ZÆHNSDORF. London: George Bell and Sons. 1880.

THE review of a work giving an account of the processes carried on in a mechanical trade may seem out of place in a journal devoted to a record of scientific progress; but books are a necessity to everyone, whatever may be the nature of his pursuits: therefore a practical treatise on a subject so important as the preservation of books must be welcome to a very large number of readers.

In the Introduction the author gives an account of the early modes used for preserving and keeping documents together, noticing the fastening of inscribed metal plates with rings, the joining of strips of vellum into a continuous roll, as in the sacred writings of the Jews, a practice continued in the synagogues at the present time. Also the most ancient form of book composed of separate leaves,—the sacred books of Ceylon, written upon palm leaves, and tied together with a silken cord passed through one end. The history is continued by describing the various ways in which vellum and paper manuscripts were placed in libraries: the bindings of some of these, as time progressed, were real works of art.

After the invention of printing books were always bound in volumes, as we are at present accustomed to see them, and in

the sixteenth century some of the finest specimens of art binding were executed. To give the reader an idea of the beauty of design of these medieval and renaissance bindings, ten photo-lithographs, from specimens in the possession of the author, are given in illustration. Impressions of tools of various periods serve as vignettes and tail-pieces, and woodcuts of machines and implements occur frequently throughout the volume.

The twenty-four chapters into which the remainder of the work is divided give a very detailed account of the various processes pursued in the binding of a book; and so minutely and carefully has this been done that a clever amateur would find no difficulty in binding books for himself. The author draws attention to a fact little known to the public, of the existence of two distinct kinds of binding, viz., "common" and "extra work." It is with the latter, or binding by hand, rather than the machine processes or "common" binding, that the writer treats upon at the greatest length.

Even those who do not wish to bind books for themselves may read Mr. Zæhnsdorf's interesting work with profit, as it shows how to distinguish good binding from bad. The remarks on the care of books and their preservation in libraries are of great value, as are also the processes for the restoration of old and injured books. A glossary of technical terms and implements concludes the work.

The author must be congratulated upon adding one to a class of books of which we have but too few; practical descriptions of any manipulative processes are far from common, and persons engaged in researches requiring apparatus of any kind must often have found difficulties in their way, and how much they needed some book of reference on working in metal, wood, glass, or other material. It would be well if those who possess mechanical skill, combined with the power of description, would follow Mr. Zæhnsdorf's example, and publish the results of their experience.

The Cobham Journals. Abstracts and Summaries of Meteorological and Phenological Observations made by Miss Caroline Molesworth, at Cobham, Surrey, in the Years 1825 to 1850. With Introduction, Tables, &c., by Eleanor A. Ormerod, F.M.S. London: Stanford.

Miss Ormerod has here placed at the disposal of the scientific world a series of observations of very considerable value. For twenty-five years Miss Molesworth made regular observations of the weather, recording maximum and minimum temperature, direction of wind, occurrence or absence of rain, and general

character of weather. The barometer and the wind-gauge were not consulted. At the end of each month the mean temperature and its relation to the average for the month, and the total rainfall are recorded. The purpose of these observations was to ascertain the influence of variation in the character of the seasons upon organic life. The leafing, flowering, fruiting, &c., of plants are recorded, and the appearances, migrations, &c., of birds, insects, and Mollusca. Of these observations many are sufficiently numerous to admit of tabulation, a task which has been performed by Miss Ormerod.

The work will be a useful manual of reference to observers of animal and vegetable life, and Miss Ormerod has done a useful and laborious task in rendering it available. She points out, however, certain shortcomings in these "Journals." Many of the native plants are mentioned merely by their local names; and as for the garden plants, which figure in the list to a considerable extent, their time of flowering, leafing, &c., must be influenced to no small extent by the manner of cultivation to which they are submitted. The insects mentioned are, as a rule, not specifically named, but merely characterised as "Aphides," "gnats," "two species of butterfly," &c.

These tables may be usefully compared with those of the Rev. Leonard Jenyns, which are based upon a much shorter series of observations.

Science Lectures for the People. Science Lectures delivered in Manchester, 1879-80. Eleventh Series. Manchester and London: John Heywood.

This series of the Manchester Science Lectures must, we regret to say, be regarded as the last. In the Preface Prof. Roscoe remarks that, in spite of the character of these discourses and the eminence of the men by whom they have been delivered, public interest, as measured by the number of persons attending, "has so far declined that the Committee have had no alternative but to discontinue the Lectures." This fact may be partly explained by the peculiar complexion of the times and the withdrawal of public interest to other spheres. Not the less is it a humiliating result.

The first lecture in this series is on "Islands as illustrating the Laws of the Geographical Distribution of Animals," by Mr. Wallace, whose name is here made to appear as "Arthur R. Wallace." The lecture contains a brief but clear and comprehensive summary of the main facts laid down in the lecturer's classical work on the distribution of animals.

Mr. Waterhouse Hawkins discoursed on the "Age of Dragons."

He explained the legendary accounts of these monsters as exaggerated descriptions of the pterodactyls. It follows as a matter of course that these creatures, if they were the archetypes of the dragons of mythology, must have still inhabited the world after the appearance of man upon the scene.

The "Physical Aspects of Palestine" were ably expounded by the Rev. Canon Tristram, F.R.S. It was pointed out that the lower valley of the Jordan and the basin of the Dead Sea, depressed as they are 1300 feet below the level of the Mediteranean, have retained, as it were, a fragment of the Miocene climate, and may have served as a refuge for certain tropical forms of life during the Glacial period. Certain it is that this district contains species quite distinct from those of North Africa, Egypt, or Arabia, and decidedly resembling South Indian and South African forms. Thus the author found in the Jordan valley a sun-bird (*Nectarinia osece*) whose nearest representatives are in the Deccan and a kingfisher (*Halcyon Smyrnensis*) not found elsewhere nearer than Madras. Many of the butterflies, locusts, and beetles of the region belong not to the Mediterranean district of the Palæarctic fauna, but to the Oriental or the Ethiopean. The lecturer very acutely argues that the Arabian Desert is older than the Sahara, because the fauna of the former has a more pronounced desert type.

Captain W. de W. Abney's "Traps to catch Sunbeams," the final lecture, deals with the action of the sun upon the earth.

Eleventh Annual Report of the United States Geological and Geographical Survey of the Territories. Being a Report of Progress for the Year 1877. By F. V. Hayden, United States Geologist. Washington: Government Printing-Office. 1879.

This volume is devoted to the territories of Idaho and Wyoming. In Fossil Entomology a rich harvest has been reaped in the Tertiary basin of Florissant, from whence 6000 to 7000 insects and 3000 plants have been already received, whilst as many more were expected by the close of the year (1877). There is every reason to believe that the study of these specimens must throw a new light on the origin and early history of insects.

Prof. Leidy has been engaged with the study of the Fossil Foraminifera of the Uintah Mountains and the Salt Lake Basin. The botanical department was conducted by two of the highest authorities in that Science, Prof. Asa Gray and Sir J. D. Hooker. Their valuable report is already before the English public.

The following remark on the practical value of Palæontology is worth recording:—"The sums which have been expended by private enterprise in the search for coal in the different States of the Union, in places where anyone possessing the merest rudiments of palæontological knowledge would have known better, is enormous, and this waste of labour and capital can be stopped only by a proper diffusion of the knowledge referred to."

We find an account of the so-called "spring" of the rattlesnake, which has been much exaggerated:—"The head, neck, and upper portion of the body, which may be raised to the height of more than a foot from the ground, are curved slightly backward, and then thrown forward with great violence. So far as we observed, the distance of the spring may amount to about two thirds the length of the snake."

A valuable feature of the volume is the Report on the Cretaceous Fossils of the Western States and Territories, by Dr. C. A. White.

A Treatise on Statics, containing the Fundamental Principles of Electrostatics and Elasticity. By G. M. MINCHIN, M.A. Second Edition. (Clarendon Press Series.) London and Oxford: Macmillan.

THE work before us, as compared with the former edition contains several important improvements. Not only has it been largely, if not entirely, freed from those misprints and clerical errors which in a mathematical book are at once so serious and so difficult to avoid, but the examples have been re-arranged in the order of their respective difficulty. Further, the demonstration of the parallelogram of forces has been based entirely on Newton's definition of force. The work is further enriched with the principal propositions of graphic statics.

The section dealing with electrostatics has been enlarged, and a chapter on Strains and Stresses has been introduced. On this subject the author remarks:—"In view of the enormous development of mathematical physics, and the wonderful inventions depending on the small strains and vibrations of natural solids which have been made within the last few years, the study of the equilibrium and motion of bodies as they are, and not as they appear in abstraction, is surely a subject of which it is impossible to exaggerate the importance. We may well ask whether, in this country, too much time is not spent in the discussion of neat mathematical unrealities—in the calculation of the behaviour of impossible bodies under impossible circumstances."

It may be safely said that these additions are decided improvements, and that this second edition will be found a decided advance on the foregoing.

Geodesy. By Colonel A. R. Clarke, C.B., F.R.S. (Clarendon Press Series.) London: Macmillan and Co.

The author commences with a sketch of geodetical surveys from Snellins, Picard, and Cassini, down to the operations of Everest and Walker in India, of Struve in Russia, and of Maclear at the Cape of Good Hope. After two chapters devoted to Spherical Trigonometry and the Method of Least Squares, he proceeds to the theory of the Earth's figure. Here it is remarked that, as regards the attraction of mountains, there is little correspondence between theory and observation, since the attraction even of the Himalayas is only perceptible at places quite close to them. Archdeacon Pratt proposes the theory that the elevations and depressions of the earth's surface have arisen from the mass having contracted unequally in solidifying, and that under mountains and plains there is a deficiency of matter approximately equal to the mass above the sea-level, whilst below ocean-beds there is an excess of matter equal to the deficiency in the ocean as compared with an equal volume of rock. On this supposition the amount of matter in any vertical column drawn from the surface to a level surface below the crust would be approximately the same in every part of the earth. We learn that the meridian of the greater equatorial diameter passes through Ireland and Portugal, cutting off a small bit of the north-west corner of Africa: in the opposite hemisphere this meridian cuts the north-east corner of Asia, and passes through the southern island of New Zealand. The meridian containing the smaller diameter of the earth passes through Ceylon on the one side of the earth, and bisects North America on the other. This position of the axes, it is said, corresponds remarkably with the distribution of land and water on the surface of the globe.

In days when such hypotheses as the growth of the earth and the secular transfer of the ocean from the southern to the northern hemisphere, and *vice versa*, are brought forward, Col. Clarke's work must be of especial value.

CORRESPONDENCE.

SOME FACTS ON ANTHROPOLOGY.

To the Editor of The Journal of Science.

SIR,—The fertility and durability of crosses between diverse human races has been, and continues to be, the subject of debate among anthropologists, and thus any little fact, the result of observation, is interesting to its professors.

During a sojourn of three years on the West Coast of Africa, the subject being interesting to me I made it a matter of observation and enquiry. In result I found the intercourse between a white man and negro woman was generally fruitful. The issue intermarrying with the issue of a similar character, generally, I may say was always productive and apparently healthy: in the second generation, on such intermarriage, the issue were weakly and unhealthy; more so in a third generation, when usually they died out. If a mulatto coon of the second generation had issue by a white or a black man, the virility of the descendants was restored. If the issue was again crossed by a white or a black man, so far as vigour and appearance went they were almost indistinguishable from the stock of the male parentage. What the result would be between a white mother and a black father I was unable to ascertain. Probably on the coast such an alliance was never formed.

Since my return to England I met with such an instance. A negro married a white woman, and had issue a son, who intermarried with a niece of the wife, and had issue three sons: the second son married a woman of the same stock as his mother, and has seven children (five girls and two boys), who are as healthy as the children of the white poor usually are; the girls have less of the negro type than the boys. The father of the children is very swarthy, and has fine curly silky hair, and a slight figure. His negro descent is unmistakable; although he has no thick lips, yet there is something about him, to those who are acquainted with the class, which would indicate the negro descent. The elder brother is a short burly figure, more negro-like than the brother just described. He married a white woman, but of an alien stock; his issue are sturdy and strong. The third son would anywhere be taken for a man without the

negro cross. He married also a woman unconnected by blood; the marriage is unfruitful.

The latter case is interesting as showing the issue of three generations through pure white mothers. The first family are not so robust as the second family: this probably is due to the intermarrying with women of the same stock.—I am, &c.,

S. B.

THE WILD BIRDS' PROTECTION ACT.

To the Editor of the Journal of Science.

Sir,—Your statements concerning the failure of this Act are, I believe, correct; but the reason is apparently because the penalty inflicted, in the few cases which have been brought before the magistrate, is too low to be of any effect. A fine of half-a-crown will never prevent such offences. Moreover, it is rumoured that the men who actually ensnare birds are merely the servants of dealers, who agree to hold them harmless in case of a penalty. The Act requires sharpening in several respects. I do not, however, think that you need expect the Society for the Prevention of Cruelty to Animals to take action.—I am, &c.,

H. T., of B.

FROST AS A VERMIN-DESTROYER.

To the Editor of the Journal of Science.

Sir,—Well-meaning people sometimes tell us that severe winters are very serviceable, by destroying noxious insects. Such, however, is not the evidence of facts. The prolonged and deadly cold of the winter of 1878-79 has not prevented the ravages of certain caterpillars from being unusually extensive. *Abraxas grossulariata* and *Plusia gamma* were exceptionally plentiful, in every stage of their existence, during the *quasi*-summer. The last winter, again, was much more severe than ordinary, but it has again proved utterly unavailing against the former of these two species, which is literally swarming.—I am, &c.,

An Old Naturalist.

THE PROTECTION OF RESEARCH.

To the Editor of the Journal of Science.

SIR,—Unless I am greatly mistaken you once suggested the formation of a Biological Defence League, to resist any further restrictions upon research. May I ask whether you have, or has any one, taken any steps in that direction? It must not be supposed that the anti-biological agitation of the last few years has died out; and probably the next Session of Parliament will not pass without the introduction of some Bill of a still more sweeping character than that which has unfortunately become the law of the land, and against which you so ably and earnestly protested.—I am, &c.,

M. D.

[We have found a strange amount of apathy on the question. We shall be very glad, however, to receive communications on this important question. Our correspondent may feel certain that nothing shall be wanting on our part to baffle the machinations of organised ignorance.—ED. J. S.]

NOTES.

Prof. Owen has communicated to the Royal Society a paper on the Ova of *Echidna hystrix*. In most respects they correspond closely with those of the *Ornithorhynchus*. The fission of the germ-mass, corresponding to that described by Barry and Bischoff in the rabbit's ovum, strengthens the conclusion that the *Monotremata* are viviparous. The functional equality of both uteri in the genus *Echidna* corresponds with the equal development of the right with the left female organs, in which it differs anatomically from the *Ornithorhynchus*.

Messrs. H. T. Brown and J. Heron have laid before the Royal Society a memoir on the "Hydrolytic Ferments of the Pancreas and Small Intestine." They conclude that the action of artificial pancreatic juice upon starch paste at 40° C. is similar to that of unheated malt-extract acting at 60° or under, the composition of the starch-products becoming comparatively stationary when 80·8 per cent of maltose has been produced. Neither artificial pancreatic juice nor the tissue of the gland itself contains any ferment capable of inverting cane-sugar. The small intestine is capable of hydrolising maltose, inverting cane-sugar, and acting feebly as an amylolytic ferment. The action of the tissue of the small intestine in bringing about these changes is far greater than that of its mere aqueous infusion, and differs materially in different regions of the intestine. This variability is independent of the relative frequency either of the glands of Lieberkühn or of those of Brunner, but appears to be correlative with the distribution of Peyer's glands. In the transition from colloidal starch to readily diffusible and easily assimilated dextrose, the actions of the pancreas and Peyer's glands are mutually dependent and complementary. The pancreas readily converts the starch to maltose, but is capable only of a very slow transformation of the resulting maltose to dextrose. Peyer's glands, almost powerless upon starch, take up the work when the pancreatic juice almost ceases to act, and complete the conversion into dextrose.

Dr. J. Burdon-Sanderson, F.R.S., and Mr. F. J. M. Page, B.Sc., have submitted to the Royal Society the results of experimental researches on the time-relations of the excitatory process in the ventricle of the heart of the frog. The facts observed agree with the following theories:—Every excited part is negative to every unexcited part as long as the state of excitation

lasts,—*i.e.*, the time it lasts in each structural element is measured by the time-interval between the beginning of the initial and the beginning of the terminal phase of the variation.

Dr. Burdon-Sanderson has also submitted to the Royal Society an instrument for investigating the successive phases of the electrical change which takes place in the excitable parts of plants and animals in consequence of excitation.

Dr. Theobald Fischer considers that the climate of North Africa might be better amended by the planting of forests than by the proposed inland sea.

General D. Ruggles, of Virginia, has taken out a patent for the artificial production of rain. It need scarcely be said that in England a process for the prevention of rain and the dispersal of clouds would be very much more useful.

Prof. J. Milne, F.G.S., of the Japanese Imperial College of Engineering, in an article in the "Geological Magazine," shows that volcanoes are chiefly distributed along the borders of land which slopes steeply beneath the sea, and gives reasons why this should be the case.

According to the comparative observations made by M. Alluard at Clermont and on the Puy-de-Dôme during the past winter, the general rule may be deduced that whenever a zone of high pressures covers Central Europe, and especially France, there is, in our climate, an interversion of the temperature with the altitude, the cold being greater in the low grounds than on the heights.

[In the terrible frost of the early morning of Christmas-day, 1860, the same phenomenon was observed in various localities in England. We learn that at Ainley Top, near Halifax, a lofty and generally cold situation, the thermometer stood 4° F. higher than at Elland in the valley below.—Ed. J. S.]

M. Inostranzeff describes a mineral carbon, from the banks of Lake Orega, much harder than anthracite, which it also surpasses in electric conductivity. If we arrange the forms of coal in the following series, lignite, bituminous coal, and anthracite, the new mineral follows after anthracite, and is still more divested of all organic character. It is not readily combustible.

Mr. Searles V. Wood, in the "Geological Magazine," asks how the glaciation of North America, as compared with that of Europe, is to be explained on Dr. Croll's well-known eccentricity theory? The superiority of temperature in Western Europe as compared with Eastern America, in corresponding latitudes, is generally admitted to be due to the existing oceanic currents. How, then, could a similar relation of temperatures prevail when these warm currents were supposed to be completely diverted from the glaciated ocean?

Dr. Phipson, in his researches upon palmelline, describes a simple method of preservation applicable to many other substances, and which he describes in the "Chemical News." It is sufficient to add a small quantity of ether to the solution of palmelline in a test-tube, cork it, and turn it over once or twice so as to dissolve as much ether as possible in the liquid, in order to preserve it with all its properties for several months. As long as the contents of the tube have a strong odour of ether, no decomposition sets in, and the optical properties of the palmelline remain intact. When decomposition occurs, the beautiful rose and yellow dichroic tint of the solution fades away, the liquid gives off an odour of ammonia, and swarms with *Bacterium*, *Vibrio*, and *Spirillium*, the latter not readily to be distinguished, save by their small size and more rapid movements, from the *Spirillium* found in the blood in cases of relapsing fever.

According to M. Tayon, ewes found in the neighbourhood of Montpellier have four teats, all yielding milk. A similar anomaly has been pointed out by Mr. Darwin, on the authority of Mr. Hodgson, in the Agia breed of sheep, at the foot of the Himalayas.

An American contemporary enlarges on the *vis medicatrix naturæ*. Would it not be well to mind the *vis deletrix naturæ* to which the late G. H. Lewes drew attention?

The McGill University, of Montreal, will shortly be provided with a large library, reading-room, and museum. Mr. Redpath, a member of the governing board, has promised to provide the necessary funds.

Mr. R. Barrett, writing in the "Victorian Review," asserts that nine-tenths of the black fellows in Australia die of consumption—a curious commentary on the practice of sending consumptive patients to Australia.

M. A. Veeder finds, from careful microscopic observation, that freezing does not free water from filth due to the presence of sewage or decomposing vegetable matter.—*Amer. Naturalist.*

Dr. Aitcheson reports that in the Kurum Valley, in Affghanistan, there is an intermingling of three very distinct floras,—those of India, Thibet, and Western Asia.

Wickersheimer's preservative fluid for animal and vegetable tissues is composed as follows:—

Alum	100 parts.
Common salt	25 ,,
Saltpetre	12 ,,
Potash	60 ,,
Arsenious acid	10 ,,

Dissolve in 3000 parts of boiling water. After cooling and filtering, add to every 10 pints of this solution 4 pints of glycerin and 1 pint of methyl alcohol.

According to Dr. Chapman, of Philadelphia, the placentation of the elephant is non-deciduate, diffuse in character, with a zonary form.

The management of the Philadelphia Academy is still a burning question in the American scientific press. The "American Naturalist" quotes a statement of the "leading officer of the Academy" to the effect that "original research was not the sole object of the Society;" and again, that "no part of the museum or library can be held in reserve for the exclusive use of any class of specialists." On these utterances our esteemed contemporary remarks that research requires the "exclusive use" of material as long as the research may last.

Mr. W. Trelease, writing in the "American Naturalist," quotes from Kirby and Spence instances of the carnivorous habits of the honey-bee.

Referring to the fertilisation of flowers by humming-birds, the same writer states that the ruby-throat visits a very large proportion of the plants of North America.

The total quantity of gold got in Victoria during the last three months of 1879 was 209,411 ounces.

Mr. F. W. Rudler announces, in the "Mineralogical Magazine," the occurrence of celestine (strontium sulphate) in the New Red Marl at Sidmouth. He describes the crystals as the finest he has ever seen in form, lustre, and transparence.

After much controversy the *Hyænodon* has been restored by Prof. Gaudry to the situation originally assigned it in 1838 by its discoverers, Laizer and Parieu, *i.e.*, among marsupial carnivores such as *Thylacoleo* and *Dasyurus*.

Dr. H. Trautschold, of Moscow, maintains that the level of the ocean is getting lower, and the total quantity of water on the surface of the globe diminishing.

It appears that, with few exceptions, Australian trees flourish as well in California as in their native country. It is hence, conversely, to be expected that the native vegetation of Calfornia can be successfully acclimatised in Australia, and that the crops which succeed in the one country are likely to do well in the other.

G. Boericke recommends perosmic acid along with oxalic acid for colouring microscopic objects. The sections or pieces of tissue are steeped for an hour in a 1 per cent solution of perosmic acid, carefully washed, and then immersed for twenty four hours in a saturated solution of oxalic acid; peculiar colourations are observed when examined microscopically in water or glycerin.

According to the "American Journal of Microscopy," silver-wire, in which the most delicate test could detect no difference of diameter, has been run through plates of rubies to the length of 170 miles.

An Exhibition illustrative of Prehistoric German Anthropology will be opened at Berlin in August. Prof. Virchow is President of the Committee of Arrangements.

The new Royal Irish University does not promise to become of great importance in Science. The Senate consists of ecclesiastics, politicians, and lawyers,—classes of men seldom favourable to the investigation of Nature.

A shower of dust was observed from April 21st to 25th, in the Departments of Basses Alpes, Isère, and Ain. It consisted of fragments of mica, garnet, and orthose, accompanied by starch grains, diatoms, and fragments of the integuments of Infusoria. There is neither native iron nor magnetic oxide, whence the dust cannot be of cosmic origin.

According to M. Des Cloiseaux the crystalline form of magnesium is a regular hexagonal prism.

A hemipterous insect (*Hysteropterum apterum*) has appeared upon the vines in the Gironde, and is occasioning injury.

Mr. J. W. Mallet, F.R.S., has communicated to the Royal Society a memoir on the "Atomic Weight and Valence of Aluminium." As the mean of twenty-five determinations he takes Al=27·019, with a probable error of ±0·0030. Further, taking the eighteen elements whose atomic weights have been determined with the greatest attainable precision, he calculates the probability that nine of these numbers should lie, as they actually do, within 0·1 of integers, supposing the value of the true numbers to be determined only by chance, and finds it only as 1 to 235·2. This example seems to show that Prout's law is not yet absolutely overturned, but that there exists a heavy and seemingly increasing probability in favour of it, or of some modification of it.

According to the Registrar-General's returns males are increasing in numbers and females decreasing. In 1879 the boys born amounted to 442,289 as against 433,577 girls. The number of females who died exceeded the males by above 30,000.

We have from time to time referred to the indiscreet dabbling of political organs in scientific matters. We find that the "Times," noticing the successful experiments of Profs. Golgi and Raggi, of Pavia, on the transfusion of blood for anæmia in the case of a lunatic, gravely states that the blood was injected into the "peritonitis"!

Dr. Huggins most truly declares that one of the great charms

of the study of Nature lies in the circumstance that no new advance, however small, is ever final. There are no blind alleys in scientific investigation. Every new fact is the opening of a new path.

Mr. Joseph Beck has recently added to the capabilities of the large model stand produced by his firm. The stage is capable of being inclined to any extent, the amount of rotation being recorded on a divided plate; or it can be turned completely over, in case it is desired to view objects without the interference with oblique illumination caused by the thickness of the stage. The swinging sub-stage bar moves independently of the mirror, or the mirror can at pleasure be attached to it, and then partakes of its motion. After adjustment of the angular direction of the light, a lever permits of its focal adjustment in the optical axis of the instrument. The whole of the illuminating apparatus, including the mirror, can be brought above the stage if desired. All the movements are registered on divided circles or scales. Provision is made for attaching a lamp to the stand of the microscope, and giving a power of motion which can be turned to good account in the examination of opaque objects. Although the arrangements of the microscope are necessarily somewhat complex, it offers unusual facilities for using illuminators of various kinds, and in a way not easily attainable with any other instrument.

Messrs. R. and J. Beck have published a description of the structures to be seen in a slide issued by them, of a section of the stem of a lime tree (*Tilia Europæa*). The section is transverse, but slightly oblique, at the junction of a branch, and double-stained, the period of growth being two and a half years. The preparation is of great beauty, and is so carefully made that almost the whole structure of the stem is capable of demonstration. The page of explanation will prove of great value to anyone who will carefully go through it, and endeavour to make out the structures described with the microscope. It is to be hoped that Messrs. Beck will continue to publish such descriptions whenever they obtain an object of educational value: such a course will do much to instruct those who would otherwise be contented merely to look at an object for its prettiness, and allow the microscope to become only a toy in their hands.

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